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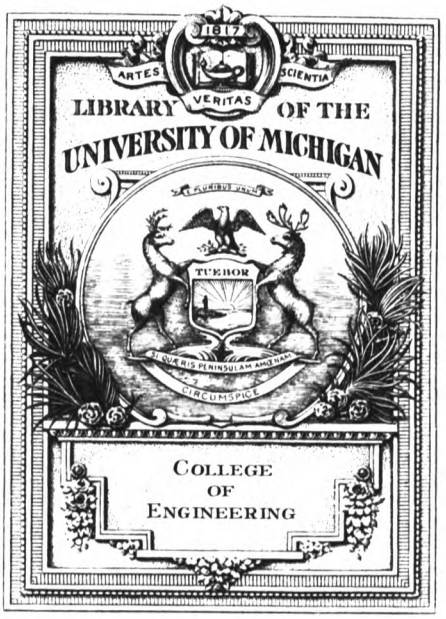

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NEW

PRINCIPLES

OF

GUNNERY:

CONTAINING

THE DETERMINATION OF

THE FORCE OF GUNPOWDER,

AND

An Investigation of the Difference

IN

THE RESISTING POWER OF THE AIR

TO

SWIFT AND SLOW MOTIONS.

WITH SEVERAL OTHER TRACTS ON THE IMPROVEMENT OF PRACTICAL GUNNERY.

BY

BENJAMIN ROBINS, Esq. F. R. S.

And Engineer General to the Honorable the East India Company.

WITH

AN ACCOUNT OF HIS LIFE AND WRITINGS,

BY JAMES WILSON, M. D.

A NEW EDITION,

CORRECTED, AND ENLARGED WITH THE ADDITION OF SEVERAL NOTES,

BY CHARLES HUTTON, LL. D. F. R. S.

And Professor of Mathematics in the Royal Academy at Woolwich.

LONDON:

PRINTED FOR F. WINGRAVE, IN THE STRAND.

1805.

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A NEW Edition of Mr. *Robins's* celebrated Treatise of Gunnery being much desired by the public, the proprietor has been induced to have it revised and improved throughout by an eminent mathematician, who has carefully revised the whole, and illustrated it with many useful notes, relative to such improvements in the art of Gunnery, as have taken place since the last edition of this work. The notes added by the present Editor are subscribed with his initial, H. to distinguish them from those of the original author.

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DOCTOR WILSON's

PREFACE.

IN publiſhing theſe tracts of my friend Mr. Benjamin Robins, *I ſhould premiſe ſome account of their author; together with ſuch reflections, chiefly relating to mathematical ſubjects, as may occaſionally ariſe.*

This excellent perſon was born at Bath *in* 1707. *As his parents were not wealthy, and alſo quakers, it was much feared, leſt the ſurpriſing progreſs he had by himſelf early made in various branches of literature, would be interrupted through want of due encouragement; eſpecially amongſt a people, who profeſs not the ſame eſteem, as the reſt of the world, for the learning they ſtyle human; ſuppoſing it not requiſite either to the underſtanding or explaining divine ſubjects.**

However, ſome particular friends of Mr. Robins *being very deſirous that he might continue his purſuits, and his merit not be loſt in obſcurity; wiſhed for this purpoſe, that he could be properly recommended to teach in this town the Mathematics, which had been one of the principal objects of his ſtudies. With this view therefore they communicated to a gentleman here a paper written by him, in order to learn what judgment perſons of knowledge might make of his abilities. This was ſhewn to Dr.* Pemberton, *who, thence conceiving a good opinion of the writer, for a farther trial of his proficiency ſent him ſome problems; of which the Doctor*

 required

* Barclay's Apology, Prop. x. § 18.

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required elegant ſolutions, not thoſe founded on algebraical calculations; adding an example of ſuch a ſolution, that the young geometer might the more readily comprehend his meaning. An anſwer was returned by Mr. Robins, *that gave a very advantageous idea of his taſte, as well as invention.*

Upon this he came to London; *where his preſence ſtill increaſed the favourable ſentiments that had been entertained of his talents. For beſides his acquaintance with divers parts of learning, there was in him, to an ingenuous aſpect, joined an activity of temper, together with a great facility in expreſſing his thoughts with clearneſs, brevity, ſtrength, and elegance; endowments, which do not always accompany ſtudious perſons. But though Mr.* Robins *was poſſeſſed of much more skill than is uſually required in a common teacher; yet being very young, it was thought proper, that he ſhould employ ſome time in peruſing the beſt writers on the ſublimer parts of the mathematics, before he undertook publicly the inſtruction of others. In this interval, beſides improving himſelf in the modern languages, he had opportunities of reading in particular the works of* Apollonius, Archimedes, Fermat, Huygens, De Wit, Slusius, James Gregory, *Dr.* Barrow, *Sir* Isaac Newton, *Dr.* Taylor, *and Mr.* Cotes. *Theſe authors he readily underſtood without any aſſiſtance; of which he gave frequent proofs to his friends. Amongſt others, one was a demonſtration of the laſt propoſition of Sir* Isaac Newton's *Treatiſe on Quadratures, which was thought not undeſerving a place in the* Philosophical Transactions.*

Not long after, an occaſion offered for him to exhibit to the public a ſpecimen alſo of his knowledge in natural philoſophy. The Royal Academy of Sciences at Paris *had propoſed amongſt their prize queſtions, to demonſtrate the laws of motion in bodies impinging on one another.*† *The celebrated M.* John Bernoulli *here con-*

* For 1727. No. 397. † In 1724 and 1726.

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*condescended to be a candidate; and though his Dissertation lost the reward, he appealed to the learned world by printing it.** *He therein endeavoured to establish M.* Leibnitz's *opinion of the force of bodies in motion, from the effects of their striking against springing materials; as Signor* Poleni *had before attempted to evince the same thing from experiments of bodies falling on soft and yielding substances. But as the insufficiency of Signor* Poleni's *arguments had formerly been demonstrated;† so Mr.* Robins *published in a journal, called* The present State of the Republic of Letters,‡ *an unanswerable confutation of M.* Bernoulli's *performance.*

It may indeed seem strange, that a mere tyro should thus overcome so redoubted a veteran. But though M. Bernoulli *must be allowed to have had a considerable share of invention in pure mathematics; yet, when any physical cause intervened, he seldom could avoid false reasonings; being deficient in that distinctness of conception, so necessary for securing against error in these more complex subjects, and which was possessed by Mr.* Robins *in a supreme degree.*

Now the recommendations of his friends, supported by such authentic testimonies of his abilities, soon procured him many scholars; amongst these, several were of real genius, who at present make an eminent figure in public affairs.

But it may be here observed, that about this time he quitted the peculiar garb and profession of a quaker; for not having the least tincture of obstinacy, superstition or enthusiasm in his nature; he soon got over the prejudices of education, and had an utter aversion to act a feigned part. However, he continued to cultivate a friendship with several deserving persons of that persuasion; not being ignorant that at all times, and in

* Discours sur les loix de la communication du Mouvement. Par M. Bernoulli à Paris, 1727.

† Pilosophical Transactions in 1722. No. 371..

‡ For May 1728.

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all places, there have been great numbers of learning, ſagacity, and even honeſty too, who through the force of early impreſſions, and a certain caſt of temper, have made moſt palpable abſurdities the objects of their faith.

Mr. Robins's *way of inſtructing was generally ſimilar to the courſe he had followed himſelf; but as he only taught perſons ſingly, and not in claſſes, it was in his power to vary his method according to the capacity or intention of each learner. However, he always began with the Elements of* Euclide, *not as interpolated by* Campanus *and* Clavius, *or anatomiſed by* Herigone *and* Barrow, *or depraved by* Tacquet *and* Dechalles; *but according to the original, handed down to us by antiquity; much leſs did he uſe any of the new modelled Elements, that at preſent every where abound.*

By what is here ſaid, I would not be underſtood to diſſuade the conſulting Clavius's *Euclide at a proper time; for as in it there is nothing inconſiſtent with the ſtrictneſs of demonſtration, ſo it contains many curious particulars relating to geometry. And indeed the contracted form into which Dr.* Barrow *has reduced the Elements, may be of uſe for refreſhing the memory, after the original has been well conſidered; the ſame judgment may be paſſed on his* Archimedes, Apollonius, *and* Theodoſius.

For want of ſuch a beginning in his ſtudies, many a mathematician, who has acquired no ſmall fame, has been altogether incapable of framing a ſynthetic demonſtration, as it ought to be, or even of readily comprehending one ſo conſtituted; but would be apt, though it had all the perfections poſſible, to imagine it tedious and obſcure, through his not being acquainted with the genuine expreſſion of the true geometry. Hence the writings of great part of the moderns on mathematical ſubjects abound with inartificial computations. The conſideration of which led Mr. Robins *often to repeat a ſaying of that elegant writer* Joannes della Faille: Mathematica multi ſciunt, matheſin pauci.*

Amongſt

* De Centro Grav. Circul. & Ellips. in the preface.

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Amongſt Mr. Robins's *ſcholars, ſuch as went afterwards to* Cambridge, *in order to qualify themſelves for one of the learned profeſſions, were wont, as is the cuſtom of young men, frequently to enter into warm conteſts with the diſciples of Mr. Profeſſor* Saunderſon, *that gentleman uſing there a very different method of inſtruction. And indeed I have met with ingenious perſons, who, though they allowed* Euclide's *Elements to be the perfecteſt book of the kind; yet did not think it the moſt proper introduction for the generality of ſtudents, at leaſt when ranged in claſſes, the way of teaching principally followed in univerſities; but the contrary of this opinion appears to be true from the conſtant and very ſucceſsful practice of the late famous Mr.* Maclaurin, *who, I obſerved with pleaſure, always begun his academical courſes with the Elements of* Euclide.*

And these Elements well deſerve to be carefully conſidered, even by ſuch as do not intend to devote much of their time to mathematical ſpeculations; for they are more uſeful, in order to acquire a habit of ſtrict reaſoning, than the moſt laboured ſyſtems of logic; that art owing in great meaſure its original, and indeed being beſt fitted for making formal anſwers, to the childiſh and ridiculous conceits of thoſe quibbling ſophiſts, whoſe impertinence Plato *has ſo juſtly expoſed; appears ſo far from being the moſt natural means of diſcovering and judging of truth, that the great maſter † of the art itſelf was a very bad reaſoner.*

The two chief grounds of falſe reaſoning are, ambiguity in the uſe of words, and principles haſtily taken up; ſcarce any one ever offending againſt the rules of mode and figure in ſyllogiſms. But the ſureſt defence againſt theſe two grounds of error, is exerciſing the mind in ſubjects, where a courſe of reaſoning is followed free from perplexity in the terms, and diſintangled from uncertainty in the principles; by which we may gain a habit

* See his Life before his Account of Sir *Iſaac Newton's* Philoſophical Discoveries. † Ariſtotle.

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habit of distinguishing between perfect reasoning and whatever in different degrees takes only the appearance of it.

Euclide *in his Elements has the advantage of a subject, the simplicity of which keeps it almost necessarily free from any ambiguity of terms, and his demonstrations are conducted with the most express design of reducing the principles assumed to the fewest number, and most evident, that might be; and in a method the most natural, as it is the most conducive towards a just and compleat comprehension of the subject, by beginning with such particulars, as are most easily conceived, and flow most readily from the principles laid down, thence by gradually proceeding to such as are more obscure, and require a longer chain of argument.*

And this great regard to perspicuity, in the method and form of reasoning, was so peculiarly the characteristic of the most ancient geometers, that Mr. Robins *chose to initiate those under his instruction in the Elements of conicks by* Apollonius, *in preference to any modern author.*

To such as had a talent for invention, he recommended the geometrical analysis of the ancients on account of is elegance, which must be allowed in many cases by the most profest admirers of algebra. Even M. d'Alembert *acknowledges,*—il y a des cas où l'usage de l'Analyse *(he means algebra)* loin d'abréger les demonstrations, les rendroit au contraire plus embarassées. De ce nombre sont entr'autres plusieurs problèmes ou theorèmes, où il s'agit de comparer des angles entr'eux.* *And Dr.* Halley, *though he had formerly bestowed the highest encomiums on algebra,† yet when he became acquainted with the geometrical analysis, gave this the preference, saying,* Methodus hæc cum algebrâ speciosâ facilitate contendit, evidentiâ vero et demonstrationum elegantiâ eam longe superare videtur: *and afterwards he adds,* Verum perpendendum est, aliud

* Encyclopédie, tom. I. p. 551.

† Philosophical Transactions, No. 205.

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aliud esse problema aliqualiter resolutum dare, quod modis variis plerumque fieri potest, aliud methodo elegantissimâ id ipsum efficere; analysi brevissimâ et simul perspicuâ, synthesi concinnâ et minime operosâ. Hoc veteres præstitisse argumento est Apollonii liber, quem impræsentiarum tibi sistimus.*

As to the principles of algebra Mr. Robins *used to deliver short precepts of his own, free from the intricacies and misconceptions, by which the generality of writers had obscured a matter in itself very plain and easy to be comprehended. The interpreting the terms of affirmative and negative, which in reality expressed only the relation of one quantity to another, as implying some absolute quality in the quantities to which they are prefixed, has occasioned all that air of mystery, by which learners are so unnecessarily perplexed. As a flagrant proof of this, see the very extraordinary account of affirmative and negative quantities in Dr.* Saunderson's *voluminous Treatise of Algebra, p.* 50 *and* 56.

Mr. Robins *explained the doctrine of fluxions, and what is usually styled the sublime geometry, after a clear and genuine manner; not having the least recourse to the absurd notion of indivisibles or infinitesimals, but as it is delivered truly, though very briefly, by its great inventor in the introduction to his admirable treatise on quadratures. What a crude idea the same Dr.* Saunderson *gave his scholars of these speculations, before Mr.* Robins *had published an explanation of them, appears from his posthumous piece, called* The Method of Fluxions, *printed at* London *in* 1756.

I shall not proceed to describe Mr. Robins's *way of instructing in the several branches of mixed mathematics; whereof he was a most perfect master, and on which he could deliver himself with the utmost clearness. I shall only observe, that as he well grounded his scholars in true geometry; it was easy for him to inform them of the practical parts in a more scientific manner, than they are handled in the vulgar treatises.*

The

* Præfat. in Apollon. de Sectione Rationis. Oxon. 1706.

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The modern authors, Mr. Robins *chiefly valued, were such as approached the nearest in their manner of writing to that of the ancients. Amongst these the great* Huygens *stands in the first place. But those who had the care of publishing his posthumous works, seem to have been of a different opinion; when they tell us, they once had thoughts of changing his real demonstrations into algebraical calculations!*

Mr. Robins *also had the highest esteem for Dr.* Barrow's *lectures, and recommended them usually to his scholars; for in those, that treat of the mathematics in general, they would find amongst other excellent things concerning the nature and principles of the science, a just defence of* Euclide, *and a full explanation of his idea of proportionality; and as the subject is handled in a popular manner, so the author, besides shewing much real learning, and exquisite choice in his authorities, has not only supported his peculiar sentiments with great subtilty of reason, but also adorned his discourse by a masculine and true eloquence.*

In his optical lectures they would see the principles of catoptricks and diopticks set forth in the compleatest manner. There the actual intersections are determined, which the rays of light, issuing from any point of an object after reflection or refraction, make with each other, as also the limits of those intersections, and how the rays, the nearer they approach those limits, are more and more constipated; whence such limits are called the foci, as the line passing through them all has been since named the caustic. Next, the author finds the principal foci in different lens's. Afterwards he treats in general of the apparent magnitudes of objects, and then particularly assigns the images of plane surfaces. The whole is intermixed with several curious propositions in geometry,*

* This Dr. *Barrow* determined in circles only; his successor in circles and in other curves by means of the radius of curvature. See *Barrow's* Optics, Lect. xiii. Art. 25, 26. Newton's Optical Lectures, Part. I. Sect. iv. Prop. 32, 33; and his Treatise of Series and Fluxions, Prob. v. §. 70.

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geometry, and confirmed by moſt accurate demonſtrations, which have been praiſed as ſuch by Sir Iſaac Newton *himſelf;* who ſuppoſed his auditors well acquainted with theſe lectures of his predeceſſor, in order to underſtand perfectly the ſublime diſcoveries, he was delivering to them.*

Dr. Barrow's *geometrical lectures alſo exhibit great marks of genius and invention. He there at the beginning diſcourſes briefly, yet clearly, about motion and time; then ſhews, from curves being generated by motion, how are deduced ſeveral of their general properties. Next he determines the tangents and areas of curves; inſerting many curious propoſitions of different kinds, as concerning the tangents and ſecants of the circle, the conic ſuperficies, &c. and in his laſt lecture he ſhews, how to determine the limits of equations, better than had been done before, or even ſince.*

I am the more particular in relation to the works of this great man, as I think they are too little at preſent regarded; and I ſhall farther ſtrengthen mine and Mr. Robins's *thoughts of them by adding, to Sir* Iſaac Newton's *judgment of the optics above quoted, the teſtimony of the famous* James Gregory *concerning both the optical and geometrical lectures; the Doctor's mathematical lectures not having been printed in his or Mr.* Gregory's *life-time. This excellent mathematician, though he had written profoundly on the very ſame ſubjects; yet in his letters to his friend Mr.* Collins, *thus candidly gives his opinion.* Mr. *Barrow* in his optics ſheweth himſelf a moſt ſubtle geometer, ſo that I think him ſuperior to any, that I ever looked upon. I long exceedingly to ſee his geometrical lectures, eſpecially becauſe I have ſome notions upon that ſame ſubject by me. I intreat you to ſend them to me preſently, as they come from the press,

* —*cum Diſſertationes, quas hic non ita pridem audivistis, tantâ rerum opticarum varietate, novarum copiâ, et accuratissimis earundem demonſtrationibus fuerint compoſitæ;*——. *Newton's* Lect. Optic. at the beginning.

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preſs, for I eſteem the author more than you can eaſily imagine.* *And Mr.* Gregory's *expectations were not in the leaſt diſappointed; for having received thoſe lectures, he writes Mr.* Collins *word,* Barrovii *(Geometricas)* Lectiones ſumma cum voluptate et attentione perlegi; atque omnes qui unquam hiſce de rebus ſcripſerunt infinito intervallo ſuperaſſe comperio.†

I need not mention Mr. Robins's *opinion of Sir* Iſaac Newton; *ſince he has had occaſion to declare it ſo often in the following tracts. And indeed Sir* Iſaac Newton's *fame ſeems at preſent to have ſurmounted all oppoſition. The philoſophers of a neighbouring nation acknowledge his merit. Though they had for years quite overlooked his book of optics; yet now they ſpeak of it in the higheſt terms.‡ And they have at length adopted attraction under the name of univerſal gravitation; which they had long rejected as an occult quality, on account its cauſe is not diſcovered. They might as well have neglected the conſideration of the laws of motion; becauſe it is not known, why motion ariſes from the colliſion of bodies; or the contemplation of the effects of light, as it has not been found out, whether it is owing to beams darted immediately from the luminous object, or to impreſſions made on a ſurrounding medium, or to ſome more latent cauſe. It is the buſineſs of the true philoſophy to explore the moſt ſimple cauſes, and from their combinations to account for the various phænomena in nature. But all the objections that could be raiſed againſt this true way of philoſophiſing, Sir* Iſaac Newton *had previouſly obviated in his works. Of this they ſeem at laſt to be ſenſible, from having conſidered them more attentively;‖ and accordingly they are attempting to make improvements on his principles.*

I am

* Dr. *Ward's* Lives of the Profeſſors of *Greſham-college*, p. 161.

† Commercium Epiſtolicum, p. 95.

‡ *Ouvrage vraiment digne de l'admiration de tous les ſiecles.* History of the Royal Academy of Sciences at *Paris* for 1752, p. 134.

‖ Memoirs of the Royal Academy of Arts and Sciences at *Paris* 1745, p. 329.

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I am very ſenſible the exactneſs of my friend in teaching is ſlighted by ſome profeſſors; they recommending Pardies, Sturmius, *and other ſhallow authors, as better calculated for the humours of many, who, ſince theſe ſpeculations have been ſo much in reputation, would gladly acquire, at little expence of time and thought, a ſmattering acquaintance with them.*

Though ſuch empirical methods have prevailed amongſt the ſuperficial and half taught; I wiſh they may not be countenanced in our public ſchools. I hope that univerſity, which has been ſo renowned for the mathematics, and all ſorts of ſound literature, will ever diſdain theſe low and imperfect ways of inſtruction in every kind of knowledge; and that the great examples of a Spenſer *and a* Milton, *of a* Bacon *and a* Harvey, *of a* Barrow *and a* Newton, *with numberleſs others, will always inſpire its ſtudents with the laudable ambition of following theſe their predeceſſors in the ſame path to glory and immortality. Not leſs ardent are my wiſhes for the proſperity of our other univerſity, whoſe learned members have ſpread far its fame in all ages; not only in the times ſince the revival of real ſcience; but even when the weſtern world lay covered under groſs ignorance, there ſhone out at* Oxford *from amidſt that thick cloud of darkneſs, bright geniuſes, whoſe works in manuſcript, as teſtimonies of their ſingular merit, are ſtill preſerved in the famous* Bodleyan Library. *And ſurely the illiberal and mechanic method of teaching theſe moſt perfect arts, will never gain ground in a ſeat of learning, whoſe profeſſors, as a caution againſt this innovation, have already obliged the lovers of theſe ſciences with editions of* Euclide *and* Apollonius, *of* Theodoſius *and* Menelaus. *As that uſeful undertaking was begun by the encouragement of the excellent Dean* Aldrich;* *ſo it is to be hoped, the preſent worthy governors of this univerſity may procure the publiſhing*

* This learned perſon, in his *Artis logicæ Compendium*, defended *Euclide* from the objections contained in a logic called *l'Art de Penſer.*

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liſhing the reſt of the ancient mathematicians. It was the multiplicity of Dr. Halley's *avocations alone, that hindered him for letting us have* Pappus *in the original; on reviſing which uſeful author he had beſtowed pains equal to his great knowledge and ſagacity.*

And here I cannot help declaring the ſatisfaction, Mr. Robins *always expreſſed on obſerving the progreſs a true taſte for real geometry made in* Scotland. *The mathematical ſciences have indeed been well cultivated by that learned nation. To a* Scotchman, *the Lord* Napier, *we are beholden not only for the admirable invention of logarithms,* and their application to trigonometry; but alſo for other very valuable improvements he has made in that moſt uſeful art, and probably his ill ſtate of health hindered his making farther diſcoveries. The incomparable* James Gregory *was likewiſe of that country, whoſe riſing merit ſeems to have given umbrage even to the great* Huygens;† *and whoſe immature death was an irreparable loſs to the mathematical world; for which loſs I have been informed, Sir* Iſaac Newton *uſed always to ſhew very ſenſible tokens of grief, whenever Mr.* Gregory's *name happened to be mentioned to him.*

In the manner above recommended, were my friend's younger days employed in promoting the knowledge of

* The Lord *Napier* was not only the inventor of the ſyſtem of Logarithms, published by himſelf, but alſo the author of that improved plan, which our countryman Mr. *Briggs*, with immenſe labour, put in execution, though Dr. *Saunderſon*, in his confuſed account of logarithms publiſhed in his Algebra, §. 392, has ſubſcribed to the miſtake, firſt propagated by Mr. *Wingate* and commonly prevailing, that Mr. *Briggs* contrived his form; notwithſtanding Mr. *Briggs* himſelf ſays expreſsly in the preface to his *Arithmetica Logarithmica*, that the form of logarithms, he computed, was communicated to him by the moſt noble inventor.

† See their diſputes in the years 1668, 1669, about the quadrature of the circle and hyperbola in the *Journal des Sçavans* and *Philoſophical Tranſactions*, as alſo in Mr. *Gregory's Exercitationes Geometricæ*. What was ſaid in that tract ſeems to have ſilenced M. *Huygens*.

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of these sciences; whose aid is in some measure required for establishing even the first foundations of civil society; as the property of each individual of the community must be ascertained in number, measure and weight.

Besides these sciences have greatly contributed to those arts, whereby the enjoyments of human life are rendered more elegant and refined.

The symmetry necessary to be observed, whether in rearing edifices for defence as well as ornament, or in delineating the forms of visible objects, is altogether owing to these sciences.

Nay, by these the very elements, whereby we subsist, are many other ways rendered capable of supplying our wants, and administering to our pleasures. Under them, even music is ranked; as its various tones arise from the different vibrations made in the air, by the regular proportions of which, concords are distinguished from discordant sounds. The influence of this art on the human mind, thereby polishing mankind, has been at all times celebrated; and it has been practised by the greatest heroes of antiquity, both sacred and profane; and the oldest system of government and religion, we know of, adopted it in the worship paid to the Supreme Being.

The firmament expanded on all sides over our heads, whether illuminated by the splendor of the sun, or adorned with the milder lights of the moon and stars, must ever have drawn the attention of a mind capable of reflecting: and these sciences, by assiduous and careful observations, have enabled us to settle the motions, and to account for and predict the various appearances of the objects, we there descry. Hence we determine the returns of the seasons, on which agriculture depends; number, and, as it were, fix the fleeting parts of time; and measure, divide and compass this globe, on which we live. By these means we become, in some manner, familiar, not only with our contemporary fellow inhabitants in far distant climes; but also with those who existed in ages long since past. Thus the

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human mind is kept clear of many extravagant and hurtful prejudices, which it would be too apt to imbibe; on our having only a ſcanty acquaintance with our own ſpecies. And the being able to determine the times, when eclipſes will happen, and the knowledge of the courſes of comets, have freed us from the terrors, that uſed to affright whole nations, ignorant of the cauſes of thoſe once alarming phænomena.

Again, from very trivial obſervations, by help of the mathematics, have been drawn moſt wonderful concluſions.

The obvious property of the common balance, and the floating of ſolid ſubſtances in fluids, have given riſe to arts, that have produced magnificent and ſtupendous works.

The broken appearance of an oar in the water, which inſtance, the academics in their idle diſputations ſo pertinaciouſly urged againſt the certainty of human knowledge, the mathematicians have laid hold of, and thence found out, not only the manner wherein viſion is performed, but how to improve that faculty; as alſo the ſurpriſing properties of light, whereby objects are preſented to us in ſuch a beautiful variety of colours; and even determined the magnitudes of the particles in matter, that are diſpoſed to reflect or refract different coloured rays.*

From bodies being attracted by the earth (for they fall to it on every ſide) has been diſcovered the law of that power, to which all portions of matter, wherever placed, do ſubmit; and how thence the celeſtial bodies are kept in their order, and perform through the boundleſs ſpace their reſpective courſes. Hence the ſtructure of the univerſe offered to us in this view, wherein we have learnt, that its different parts, even the moſt remote, are all governed ſo completely by one and the ſame ſimple principle, muſt inſpire a juſt and awful idea of its moſt powerful, wiſe and beneficent author; and baniſh

* Ciceron. Academ. L. iv. §. 7, 25.

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baniſh from our minds every unworthy conception of him, that ſuperſtition and enthuſiaſm may ſuggeſt.

Beſides theſe apparent advantages, accruing to mankind from cultivating the mathematical ſciences, the many ſteps of pure reaſoning, that are gone through in the progreſs of knowledge, from the moſt obvious affections of magnitude, to the admirable properties that are now diſcovered, ſince theſe ſciences have been ſo happily applied to the ſtudy of nature, muſt afford an exquiſite pleaſure to a mind duly adapted for ſuch reſearches. And perhaps in theſe mental ſatisfactions is to be found our ſupreme felicity. But as we are of different tempers, ſo that what delights one, appears inſipid to another; it is too frequent a failing, even amongst perſons of true learning and genius, to overrate their own purſuits, and contemn thoſe of others. But it is the buſineſs of experience and reflexion to wean us of theſe unreaſonable prepoſſeſſions, whereby we may become more cautious in paſſing too raſh a judgment on ſubjects generally allowed to be uſeful, and which we ſlight, chiefly becauſe we have not maturely conſidered them, or perhaps have wanted talents to make ſuch inquiries.*

It has indeed been frequent at all times, even for writers not deſpiſing any branch of learning, yet in praiſing the art in which they excelled, to undervalue in ſome meaſure that of others. Thus long ago, though Cicero *in many places of his works ſpeaks highly in commendation of the mathematics; yet at the beginning of his treatiſe* De Oratore, *where he would exalt his own profeſſion, by ſhewing how few had attained perfection in it, has theſe words:* Quis ignorat, ii, qui mathematici vocantur, quanta in obscuritate rerum et quàm recondita in arte, et multiplici, subtilique versentur? quo tamen in genere ita multi perfecti homines exstiterunt, ut nemo ferè studuisse

* Eodem modo de Aristotele et Isocrate judico quorum uterque suo studio delectatus contempsit alterum. *Cic. Offic.* at the beginning.

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ei scientiæ vehementius videatur, quin, quod voluerit, consecutus sit. *And but the other day a moſt celebrated author, who at times has treated the mathematics very civilly: yet in ſetting forth the difficulty of his own happy talent, has thought fit to ſay;* il est aisé d'apprendre la trigonométrie.* *But theſe writers have not in this caſe, as they ought, taken care in making theſe ſorts of compariſons, to diſtinguiſh between ſuch as had acquired a knowledge only in theſe arts, and ſuch as have produced diſcoveries. There have been but few* Archimedes *and* Newtons: *though it may not be difficult to underſtand trigonometry, it is not ſo common to make real and elegant improvements in that, or in any other branch of the mathematics.*

In thus apologiſing for my friend's profeſſion, I would not be underſtood not to entertain a due eſteem for other parts of learning, or in the leaſt to place the mathematics in competition with them.

After what has been ſaid, it would be needleſs to take notice of ſuch, as without any taſte, and being wretched reaſoners, have ſpoken with contempt of the mathematics. Thus Joseph Scaliger, *when convicted by* Clavius *of the groſs parallogiſms, he had committed in attempting to ſquare the circle, not only abuſed his monitor, but his vanity rendered him ridiculous enough as to aſſert* præclarum ingenium non potest esse magnus mathematicus;† *and on a like account this ſelf-ſufficient critic, ſtyled our all accompliſhed Sir* Henry Savile, un certain orgueilleux sot.‡ *But that learned knight, who had been thoroughly acquainted at* Paris *with* Scaliger's *failings in point of reaſoning, afterwards gave his real character in his Prelections on* Euclide.§

From the above weakneſſes of mind Mr. Robins *ſeemed to be exempt by nature; having been endued with*

* Preface to a comedy called *Le Caffé ou l'Ecossaise*, p. ix. printed in 1760.

† Scaligerana, article Clavius.

‡ Scaligerana, article Casaubon.

§ Lect. ii. p. 22. Lect. iv. p. 73. and Lect. xii. at the end.

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with a capacity fitted for very different ſpeculations. Hence, though he profeſſed teaching the mathematics only, he would however ſometimes aſſiſt particular friends in other parts of knowledge; for he was well qualified to point out the real beauties of writers in all ſorts of polite learning, and alſo the excellencies in the performances of great artiſts, as his taſte and judgment were not limited to a ſingle ſubject alone, but extended equally to hiſtory, oratory, poetry, muſic, architecture, ſculpture, painting, and works of genius and invention in every kind.

Notwithſtanding he was thus fraught with variety of knowledge, he never made any oſtentatious uſe of it; his converſation being always lively and entertaining, generally on gay and joyous ſubjects, without the leaſt mixture of pedantry or affectation of any ſort. However, when a proper occaſion preſented, he could explain himſelf on the moſt abſtruſe points with great clearneſs and ſtrength, both of reaſon and expreſſion.

But the confinement in his profeſſion not ſuiting with his active diſpoſition, he gradually declined it, and went into other courſes of life, that required more exerciſe; for which he was fitly adapted, as being of a middle ſtature, well made, and not corpulent. Hence he tried many laborious experiments in gunnery; believing the reſiſtance of the air had a much greater influence on ſwift projectiles, than was generally ſuppoſed. Hence he was led to conſider thoſe mechanic arts, that depended on mathematical principles, wherein he might employ his invention; as the conſtructing of mills, the building of bridges, draining of fens, rendering rivers navigable, and making of harbours. In which inquiries he received conſiderable advantages from a very early acquaintance with William Ockenden, *Eſq. who having a peculiar genius for ſuch ſpeculations, and a moſt intimate friendſhip always ſubſiſting between them, they mutually improved each*

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other, by a conſtant communication of their thoughts and obſervations on theſe ſubjects.

Amongſt other arts of this kind, fortification very much engaged Mr. Robins's *attention; wherein he met with opportunities of perfecting himſelf by the view of the principal ſtrong places of* Flanders, *in ſome journeys he made abroad with perſons of diſtinction and fortune.*

On his return home from one of theſe excurſions, he found the learned here amuſed with a treatiſe, written expreſsly againſt the mathematicians, intituled The Analyst. *This was the production of Dr.* Berkley, *biſhop of* Cloyne; *who had been long famous for vain and metaphyſical paradoxes; and his objections to Sir* Isaac Newton *aroſe chiefly from his not underſtanding the writings of that great man.*

Mr. Robins *therefore was adviſed to clear up this affair, by giving a full and diſtinct account of Sir* Isaac Newton's *doctrines, in ſuch a manner, as to obviate all the objections, without naming them, that had been advanced by the author of the* Analyst; *ſo that there need no recourſe be had to the biſhop's book to underſtand the ſubject, and alſo that he himſelf, if poſſible, might ſee the weakneſs of what he had, with ſuch an air of aſſurance, urged. Accordingly Mr.* Robins *printed, in* 1735, A Discourse concerning the Nature and Certainty of Sir *Iſaac Newton's* Methods of Fluxions, and of prime and ultimate Ratios.

This ſmall tract is admirable, on account of its clearneſs, brevity, and ſtrength of expreſſion.

Towards the illuſtrating the nature of fluxions, it is there at the beginning obſerved, that as the fluxion of a line is the velocity or degree of ſwiftneſs of a point deſcribing in its motion that line; ſo the fluxions of ſurfaces and ſolids may be expreſſed by the velocities of points deſcribing lines, that increaſe proportionally with thoſe quantities.

Next

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Next the fundamental properties of fluxions are demonſtrated after the manner of the ancients, in the moſt rigid form; and it is ſhewn how to apply them to the determining the tangents and areas of curves. Then is given a full and clear account of the ſeveral orders of fluxions, or the variations in the velocities of increaſing and decreaſing; and the uſe of ſecond fluxions is illuſtrated in the inveſtigating from a new conſideration the curvature of curves. As here, in demonſtrating the fluxion of a power, Sir Isaac Newton's Binomial Theorem *is introduced; ſo in the* Present State of the Republic of Letters, *for* October 1735, *there is publiſhed a demonſtration of it independent on that theorem.*

After follows a very diſtinct relation of Sir Isaac Newton's *method of prime and ultimate ratios, and how it may be applied to the purpoſes, for which it had been ſhewn fluxions might be uſed; and there is given another new way of determining the curvature of curves. Here is fully made out the connexion between this method, and that of the ancient geometers, called exhauſtions; ſo that it may be of no ſmall ſervice to young mathematicians, for their more ready perceiving the force of the demonſtrations in the* Principia.

The laſt part illuſtrates Sir Isaac Newton's *way of computing the fluxions of quantities; concluding with an explanation of what he had delivered concerning their* Momenta. *And this particular is ſtill farther proſecuted in the abovementioned journal.*

Notwithſtanding this tract was written with the greateſt perſpicuity, completely explaining the genuine ſenſe of Sir Isaac Newton's *doctrines; yet there were found ſome, who were not ſatisfied with what Mr.* Robins *had publiſhed.*

And this was the leſs to be wondered at, ſince many of our mathematicians having firſt learnt to apply the computations of this method, to geometrical figures, from the Marquis de l'Hopital's Analyse des infi-

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niment petits, *they were inſenſibly infected with the principles of infinites followed in that book.*

One amongst theſe, who under the name of Philalethes Cantabrigiensis, *had on this wrong foundation undertaken the defence of the mathematicians againſt the author of the* Analyst, *continued to maintain his miſtaken repreſentation. This induced my friend to write two or three additional diſcourſes; which, as they may poſſibly contribute to the eaſier apprehending Sir* Isaac Newton's *genuine meaning, I have here reprinted. The ſubſequent peeviſh and ungentleman-like ſhifts,* Philalethes *condeſcended to, rather than anquieſce in Mr.* Robins's *ſuperior knowledge, I would gladly have paſſed over in ſilence; had not I found Mr.* Robins *highly blamed by M.* de Buffon, *in the Preface to a* French *tranſlation of Sir* Isaac Newton's Method of Series and Fluxions. *I have therefore endeavoured at a vindication of my friend. But as this would now carry me too far from the deſign of the preſent preface, I have given it with other things in an* Apendix *to the following tracts.*

In 1738 *he defended Sir* Isaac Newton *againſt an objection, contained in a note at the end of a tract in* Latin, *called* Matho sive Cosmotheoria puerilis, *written by one Mr.* Baxter, *who had before publiſhed no ſmall volume concerning the ſoul! But what ought we to think of this man's capacity for reaſoning, who could not ſee his error in a mathematical ſubject, though it had been ſo plainly ſhewn him; as we learn by an enlarged edition he afterwards made of his* Matho *in* English, *that he ſtill perſevered in his miſtake?*

And indeed the world has never wanted preſuming men, that fancied themſelves able to diſcuſs inexplicable points; yet when they have attempted ſuch as are capable of demonſtration, they have manifeſted their deficiency in reaſoning. It is true, the greateſt genius may through inattention fall into parallogiſms, even in geometry.

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geometry. But here lies the difference. The one on the leaſt hint given of ſuch a ſlip, readily of himſelf rectifies it; but the other, if you offer to ſet him right, only flounders on in his errors, being not able to comprehend the force of your objections, however clearly urged; nor at length even his own meaning, having never had a juſt idea of the ſubject he unadviſedly undertook.

Mr. Robins *printed, in* 1739, Remarks on *M. Euler's* Treatise of Motion, and on the Compleat System of Optics, *written by Dr.* Smith, *the preſent worthy maſter of* Trinity-College, *in the univerſity of* Cambridge; *as alſo on Dr.* Jurin's Discourse of distinct and indistinct Vision, *that was annexed to Dr.* Smith's *work.*

One purpoſe of this piece is to ſhew the errors mathematicians are liable to, by implicitly adhering to their algebraical calculations. The latter part is written after a free manner; for which the author in his preface has given his reaſons. This alſo, as well as the diſcourſe on fluxions, occaſioned on the ſide of Dr. Jurin *a controverſy, which might have proved of long duration; had it not been cut ſhort by Mr.* Robins *being then called to a public employment.*

As all the tracts, to which he put his name, were confined to the mathematics; the clearneſs, brevity and elegance, wherewith they were compoſed, could be judged of by a very few, and indeed perfectly only by ſuch as were not unacquainted with the writings of the ancient geometers. But in ſome anonymous pieces, he publiſhed on more popular ſubjects, greater numbers were ſenſible of the force of his reaſoning, the juſtneſs of his deſcriptions, the beauty of his expreſſions, and the harmony of his periods.

In the year 1739 *there came out three pamphlets, which acquired him great reputation, though they were written very haſtily; as the incidents, that occaſioned them, were ſudden and urgent.*

The

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The first was intituled, Observations on the present Convention with *Spain. Here the specious veil, with which some had endeavoured to cover the meanness of this transaction, was entirely removed; and all the invincible arguments against it set in the strongest light, which very arguments were afterwards made use of, when it became matter of debate in parliament.*

The second was called, A Narrative of what passed in the Common Hall of the Citizens of *London* assembled for the Election of a Lord Mayor. *This, though composed indeed on a less momentous affair, yet contained in it surprising strokes of true oratory.*

The third was written on the following occasion. Many eminent patriots, as they were then styled, upon the sanction given by the House of Commons to the Spanish *convention, notwithstanding all their weighty speeches and reasons against it, became so disgusted, that they took a resolution from that time of not attending the business of parliament; which proceeding, called a secession, was highly resented by the other side; and the seceders at length returned as usual to their seats in the house. This defection being by many deemed rash, Mr.* Robins *was requested to write an apology for it. The pamphlet he composed was delivered to a principal person concerned, in order to be transcribed, and the original destroyed, the better to conceal the real writer; whose condition in life might not be able to secure him from the resentment, the freedoms taken in it might provoke. After some alterations, to soften matters, were made, and a preface prefixt, of both which Mr.* Robins *was neither the author nor approved; it was at length published under this title;* An Address to the Electors and other free Subjects of *Great Britain,* occasioned by the late Secession. In which is contained a particular Account of all our Negociations with *Spain,* and their treatment of us for above ten Years past.

This

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This tract, though by the abovementioned alterations in a manner disfigured, was universally esteemed, and for some time, as well as the Observations on the Convention, *generally reputed to have been the production of that great man himself, who was at the head of the opposition to Sir* Robert Walpole; *but it proved of such consequence to Mr.* Robins, *that it occasioned him to be employed in a very honourable post. For the patriots continually gaining ground, and at length a majority; Sir* Robert *was upon this turn of affairs advanced, for his better security, to the peerage, by the title of Earl of* Orford. *However, a committee of the House of Commons being appointed to examine into his lordship's past conduct, Mr.* Robins *was chosen their secretary.*

And here he exhibited constant proofs of his fitness for this important station, in taking down very readily and perfectly the examinations made before the committee; which was no easy task, as the persons examined were well-versed in secret transactions, and greatly interested, that the real truth should not be come at. After the committee had presented two reports of their proceedings, a sudden stop was put to their farther progress by a compromise between the contending parties; and most concerned were gratified, either with honours or places, except the secretary; which some attributed to his having been too sincere in the affair. However, in 1742 *the second report clandestinely came abroad, and was much admired.*

Thus Mr. Robins *being again at leisure, printed the same year a small treatise, intituled* New Principles of Gunnery, *containing the result of many experiments he had made, whereby is discovered the force of gunpowder, and the difference in the resisting power of the air to swift and slow motions; whence it plainly appeared, that the opposition of that medium to bullets and shells, shot from cannon and mortars, far exceeded what was generally imagined; and that the track described in their motion differed from that of a paraboli-*

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cal line, to a degree undiscovered by any who had written expressly on the subject, from the days of the famous Galileo, *to the present time.*

Sir Isaac Newton *indeed, as Mr.* Robins *observed,** *was very sensible of the effects of this resistance; and has proposed how to assign in particular cases a curve of a different species from the parabola, as more answerable to the projectile's motion.*† *Nor was this great man wholly unapprised of the increase of resistance, both from the rotatory motion of the moving body, and also from the pressure of the fluid on the body, becoming by its motion greater on the fore part than on the hinder.*‡

The tract of Mr. Robins *was preceded by an account of the progress modern fortification had made from its first rise; as also of the invention of gunpowder, and of what had already been performed in the theory of gunnery.*

This was well received by the public, and, it was thought would have procured the author preferment; if he never had been secretary to the secret committee.

Upon a discourse containing certain experiments being published in No. 465 of the Philosophical Transactions,

* Preface, p. 51.

† Princip. Lib. II. Prop. x.

‡ Ibid. Prop. xl. in the Schol. in the Remarks on Experim. 12. Amongst Mr. *Robins*'s memorandums is the following passage, which seems as intended to have been inserted into a preface to his discourses, that had been read before the Royal Society, and which he was about publishing. "Since what I "wrote in a former treatise on this head, I find Sir *Isaac Newton* "had considered this, though not in the present instance; for "in speaking of some conjectures he had made on the separa- "sion of the colours by the prism, he adds,"—*I remembered that I had often seen a tennis ball, struck with an oblique racket, describe a curve line. For a circular as well as a progressive motion being communicated to it by that stroke, its parts on that side, where the motions conspire, must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and re-action of the air proportionably greater.* Philosophical Transactions, No. 80. p. 3078. Feb. 1671-2.

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actions, *in order to invalidate ſome particular opinions Mr.* Robins *had advanced; he thought proper, in an account he gave of his book in the ſame Tranſactions,* No. 469, *to take notice of thoſe experiments. And in conſequence of this, ſeveral diſſertations of his, on the reſiſtance of the air, were read, and the experiments exhibited before the Royal Society in the years* 1746 *and* 1747; *for which he was preſented with a gold medal.*

On that occaſion Mr. Folkes *the preſident addreſſed the Society and Mr.* Robins, *as follows.*

GENTLEMEN,

The curious and valuable experiments, which have lately been made before you, by our very worthy brother of the Society, Mr. *Benjamin Robins*, concerning the reſiſtance given by the air to bodies in motion; particularly to military projectiles, and ſuch others as are made to paſs through that medium with great velocities; could not eſcape the attention of my honoured predeceſſor, your late preſident, Sir *Hans Sloane*: who in his preſent retirement from buſineſs, ſtill applies himſelf with unwearied diligence, to all ſorts of learned and philoſophical enquiries.

He has ſtill the ſame concern for the proſperity and for the honour of this body: and the knowledge and information he daily receives of every thing remarkable that paſſes amongſt us, or that is communicated to us from without, affords him no leſs ſatisfaction, than when the weight of fewer years, and a more vigorous ſtate of health, allowed him to give ſo conſtant and ſo regular an attendance at our meetings, during ſo long a period of time, and through the ſeveral offices he has held in the Society.

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As I say the before mentioned experiments could not escape his notice, so neither would he let them want the sanction of his own approbation: and he has therefore this year, as the surviving executor of the late Sir *Godfrey Copley*, nominated Mr. *Robins* to receive the annual prize medal of gold, bestowed by the Society in consequence of Sir *Godfrey's* benefaction.

I accordingly, at a late meeting of your council, acquainted the gentlemen there present with this appointment: who were unanimously pleased to approve of the same, and to put into my hands a medal, upon which, according to their order, I have caused Mr. *Robins's* name, and the date of the present year to be engraved.

It is from these experiments, and from those others which Mr. *Robins* is still preparing to exhibit, that we may expect to see compleated the whole, and the true theory of projectiles. What *Galileo* and *Torricelli*, who first demonstrated the motions of these bodies *in vacuo*, knew to be still wanting in their theories, will hereby be supplied: and these particulars will at last become known, which they wished that future observers would make diligent and careful experiment about.

The great Sir *Isaac Newton*, who did so much honour, when living, not only to this society, and to this chair; not only to this country, and to the age he lived in, but to the world in general, and to human nature itself: this great man, I say, in his admirable *Principia*, investigated the laws of the resistances made to bodies in motion, during their passage through the air and other fluids, and those upon different theories, and upon different suppositions. He also made experiments upon the resistance given to funipendulous bodies in their oscillations, and to others in their fall, which he caused to be dropped for that purpose from the highest part of the cupola of St. *Paul's* church:

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church: but he never had the opportunity of making trials, upon those much greater resistances, that shells and bullets are impeded by, in those immense velocities with which they are thrown from military engines.

And hence it has come to pass, that succeeding writers, even those of the first class, and who are the most justly distinguished by their great knowledge and abilities, not sufficiently attending to the true theory of these motions, have been of opinion, *that in large shot of metal, whose weight many thousand times surpasses that of the air, and whose force is very great, in proportion to the surface wherewith they press thereon, this opposition is scarce discernable, and as such may, in all computations, concerning the ranges of great and weighty bombs, be very safely neglected.*

This is one of those principles, which the learned gentleman, who favoured us with these experiments, very particularly proposed to examine, and that both theoretically and practically; and he has accordingly shewn, by a series of the most curious and most ingeniously contrived experiments; if not the absolute quantities of these resistances in all cases, at least that they are enormously great, much beyond what any former theories had assigned, and such as are absolutely necessary to be considered in all strict reasonings concerning these matters: particularly, as they in so remarkable a manner curtail and diminish the great ranges of all sorts of cannon and mortar-pieces.

He has also by the way had occasion to take notice of several new and surprising *phænomena*, attending these sorts of motions: such as the different resistances, that are given by the same medium, to one and the same body, when put into motion with the same velocity, and when presenting

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ing to the resisting *medium*, the same or an equal *superfices*, but only in a different direction.

Mr. *Robins* has yet farther pushed his trials, to certain deflexions, hitherto entirely unconsidered, of bullets and other projectiles from the vertical plane in which the shot is made: and which he has with great subtilty accounted for, from a rotatory motion that bullets accidentally acquire about an *axis:* and as a confirmation of his theory, he has in many cases been able either to prevent this deflexion, or to direct it such way as he thought proper.

The last particular I shall here take notice of, is a most extraordinary, and astonishing increase of the resistance, and which seems in a manner to take place all at once, and this when the velocity comes to be that of between eleven and twelve hundred feet in one second of time. This increase however only concerns the absolute quantity of the resistance, the law of it continuing in other respects nearly the same as before: and it is remarkable farther, that the case wherein this increase of resistance becomes observable, is that, wherein the velocity of the shot, is at least equal to that velocity with which sounds are propagated: whence Mr. *Robins* has with great sagacity offered his reasons to believe, that in this case the air does not make its vibrations sufficiently fast, to return instantaneously into the place the bullet has left; but that the bullet then leaves a vacuum behind it; whereby it becomes exposed to the whole resistance, the body of air before it is capable of giving.

Should I but barely enumerate all the particulars in these experiments, that have appeared to myself both curious and instructive, I must by far exceed the bounds, that can reasonably be allowed me on the present occasion. I shall therefore only add, that as I cannot sufficiently admire the elegance

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elegance and the judgment, with which the gentleman's experiments have been contrived and conducted; so neither can I enough commend the laudable and indefatigable pains he has taken, in making so very many experiments himself, and in collecting also so many others from elsewhere; all which he has deduced such computations from, as might enable him to compare the same with, and hereby to confirm and ascertain his theories.

Mr. ROBINS,

It is now, Sir, with the greatest satisfaction that I can assure you of the high esteem the Royal Society have for you, and of the just value they set upon your very curious and useful communications. It is by their command, and in their name, that I put into your hand this faithful token of their regard: in which you will not attend to the smallness of the gift, but consider it as it comes from a Society, neither abounding in sums of silver or gold, not pursuing or coveting worldly riches, but the improvement only of philosophical knowledge. You will please therefore in such a light to accept this medal, and in some sort to compare it to those crowns, that were given to eminent persons, in the first ages of simplicity of the ancient *Greeks*; and which although only wreaths of olive, or even garlands of grass, were not on that account the less esteemed by those upon whom they were bestowed, as they were still authentic testimonials, of the most exalted virtue, and of the most distinguished merit.

As thus Mr. Robins's *experiments and theories met with the greateſt approbation from the beſt judges here; ſo the tract he had publiſhed on thoſe ſubjects did*

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him much honour abroad; for the famous M. Euler *tranſlated it into* High Dutch; *accompanying this ſmall piece with an immenſe commentary* and beſtowing praiſes on it, yet not without attempting to diſcover errors. This was printed at* Berlin *in* 1745, *and Mr.* Robins *ſoon after informed me, that M.* Euler's *principal objections aroſe from miſtakes; the ſource of which having found out, he intended to publiſh an anſwer; but from that time continual interruptions prevented him.*

It has alſo been tranſlated into French *by M.* Le Roy;† *and it is often mentioned with applauſe in the Literary Journals of* Europe.‡ *The authors of one of them in giving an account of M.* Du Hamel's *experiments on the ſame ſubject, ddd*---il nous expose à cette occasion quelques experiences très-curieuses faites par d'autres physiciens particuliérement par M. Robins, que l'Angleterre comptoit au nombre de ses plus gran géometres, mais que la mort lui enleva il y a déja quelques années.‖

The reputation, ſo juſtly acquired by this performance, made a foreign profeſſor of the mathematics, when in London, *pay him a viſit; and his eſteem for Mr.* Robins *was by that interview greatly improved, inſomuch that on his return home, he commended Mr.* Robins *ſo effectually to the late prince of* Orange, *that he was invited over to aſſiſt in the defence of* Berghen-op-Zoom, *then inveſted by the* French; *and he did accordingly ſet out for that place; but it was entered by the beſiegers,* September 16, 1747, *juſt after his arrival in the* Dutch *army.*

As

* This celebrated work has lately been translated into English, by order of the Board of Ordnance, with many necessary Remarks and useful Tables, by Mr. Hugh Brown of the Tower, published in one volume, in quarto.

† Memoir. de l'Academie de Sciences à Paris, for 1751, p. 45.

‡ Journal des Sçavans, Mai 1743. Nov. Acta Erudit. Mai 1746. Mem. de l'Acad. des Sciences à Paris, 1750, p. 1. Mem. des Sciences & Belles Lettres à Berlin, ann. 1755, p. 104.

‖ Journal des Sçavans, Jan. 1755.

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As Mr. Robins *had at this time no opportunity of ſhewing his ſkill in military affairs; ſo ſome years before he was diſappointed in his hopes of ſerving that way his own country. For in* 1741, *Lord* Cathcart *was to go on a ſecret expedition, which was generally believed to be intended againſt the* Havanna. *And Mr.* Robins *having had a manuſcript plan of it, drawn upon the ſpot with great exactneſs, given to him by ſome of his friends, immediately applied himſelf to conſider, how the ſiege of this city, now delineated before him, might beſt be attempted; and by great pains in inquiring of numberleſs perſons, who had been there, got ſuch information, as enabled him to write a very accurate ſcheme for the conqueſt of that important fortreſs; in which ſcheme, after having deſcribed the circumjacent country, and pointed out the propereſt place, where the troops might land, he then ſet down a very particular account, in what manner they ought to proceed, ſtep by ſtep, through all their attacks, firſt againſt the Moorcaſtle, and then againſt the body of the town itſelf.*

This ſcheme, together with the manuſcript plan juſt mentioned, Mr. Robins *preſented to Lord* Cathcart, *not without hopes, that his Lordſhip would thereby be induced to allot him a ſhare in the execution of an enterprize, he had ſo fully marked out, as hardly to omit a ſingle circumſtance, that ſtood in need of explanation: but his expectations were totally diſappointed; for notwithſtanding the preſent was received with great readineſs, and many encomiums beſtowed upon the compleatneſs of the performance, yet his Lordſhip was quite ſilent with regard to engaging his perſonal aſſiſtance. Many people then judged this coldneſs to proceed from Mr.* Robins *having been attached to the wrong ſide of political party; but time ſoon diſcovered* Carthagena, *and not the* Havanna, *to be the great object of that unſucceſsful expedition.*

Some years after this diſappointment, Mr. Robins *had the good fortune to be engaged in a work, that proved of more conſequence to him, than all he had hitherto written.*

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In 1741, *Mr.* Anson; *(now Lord* Anson, *and at the head of the* Admiralty, *when our fleets carry terror wherever they appear,) as commodore in the* Centurion *man of war, accompanied with other ships, began his voyage round the world, which though by disasters had not all the success that might have been reasonably expected, yet under this great commander were produced many brave and skilful officers, who at present do so much honour to the* British *navy. Of this voyage the publick had for some time been in expectation of seeing an account composed under his Lordship's own inspection. For this purpose the reverend Mr.* Richard Walter *was employed, as having been chaplain aboard the* Centurion *for the greatest part of the expedition. Mr.* Walter *had accordingly almost finished his task, having brought it down to his own departure from* Macao *for* England; *when he proposed to print his work by subscription. Then Mr.* Robins *being recommended as a proper person for reviewing it; on examination (notwithstanding the shortness of the time, that could be allowed for such an undertaking) it was resolved, that the whole should be written entirely by Mr.* Robins; *what Mr.* Walter *had done, being, as Mr.* Robins *informed me, almost all taken verbatim from the* Journals, *was to serve as materials only. And upon a strict perusal of both the performances, I find Mr.* Robins's *to contain about as much matter again as that of Mr.* Walter: *and indeed the introduction entire, with many dissertations in the body of the book, were composed by Mr.* Robins, *without having received the least hint from Mr.* Walter's *manuscript; and what he had thence transcribed regarded chiefly the wind and the weather, the currents, courses, bearings, distances, offings, soundings, moorings, and the qualities of the ground they anchored on, with such particulars as generally fill up a sailor's account. So this famous voyage was composed, in the person of the* Centurion's *chaplain, by Mr.* Robins, *in his own style and manner. Of this Mr.* Robins's *friends, Mr.*

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Glover *and Mr.* Ockenden, *are witneſſes as well as myſelf, we having compared the printed book with Mr.* Walter's *manuſcript.*

And this was at that time no ſecret; for in the counterpart of an Indenture, *now lying before me, made between* Benjamin Robins, *Eſq. and* John *and* Paul Knapton, *bookſellers, I find, that thoſe bookſellers purchaſed the copy of this book from Mr.* Robins *as the ſole proprietor, with no other mention of Mr.* Walter, *than a proviſo in relation to the ſubſcriptions he had taken.*

Thus as many of Mr. Robins's *ſmaller pieces came abroad without a name; ſo this larger volume was printed in the year* 1748, *under that of another. But though Mr.* Walter's *appears in the frontiſpiece; yet Mr.* Robins *was ſo well underſtood here to be the principal author, that he was univerſally congratulated on its ſucceſs. And indeed no production of this kind ever met with a more favourable reception from the public; four large editions were ſold off in leſs than a twelvemonth; and it has been tranſlated into moſt of the languages of* Europe; *and it ſtill ſupports its reputation, it being this year,* 1761, *printed here for the ninth time.**

Though the beauty of the original cannot be fully perceived by a foreigner, yet the Journals abroad have ſpoken of it with great applauſe. The authors of one of them† declare—aussi croyons-nous avec lui que son ouvrage est très-superieur à tous ceux qui ont paru dans ce genre; *and on mentioning the ſufferings of the mariners in one part of the navigation, they ſay,* La peinture de ces divers accidens est si forte & si vive, qu'elle porte l'effroi & la commiseration dans l'ame des lectures. *And M.* d'Alembert, *in his* Disquisition *on the figure of the earth, recounting the voyages that had been made round it, ſays,* En der-

 niere

* The fifth edition at *London* in 1749 was revised and corrected by Mr. Robins himself.

† Journal des Sçavans Janv. 1750.

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niere lieu ce voyage a été fait par l'amiral Anson, dont on a imprimé la relation si interessante et si curieuse.*

We are also told† *that in a treatise published at* Paris *in* 1756, *intituled* Histoire des Navigation aux Terres Australes, *there are extracts from it; and accordingly the authors of the* Memoires des Trevoux‡ *in their account of that* Collection, *after having duly commended two famous voyages*§ *to those parts, add, that in them,*—on ne sent pas cette chaleur d'imagination, cette vivacité de style, qui caracterisent l'histoire de l'expedition dont se chargea l'amiral Anson en 1741.—Richard Walter, chapelain du vaisseau amiral, qui a écrit la relation de ce voyage, y peint une flotte qui s'embarque avec allégresse, qui vogue avec les plus hautes espérances, et qui au Détroit de la Maire, et même à l'entrée de la Mer Pacifique, voit la fortune dont elle se repaissoit s'evanouir dans le sein des plus affreux desastres.—Tout ce que l'un et l'autre élément peuvent offrir de plus riant et de plus effrayant, se rencontre sous le plume de Walter et varie agréablement ses curieuses observations sur la géographie la physique et la navigation.

I have chose to set down rather these foreign testimonies to the advantage of Mr. Robins's *performance, than to enlarge myself in its commendation; since the inviolable friendship, that is well known to have subsisted above twenty years between us, may be thought to influence my opinion.*

Mr. Robins, *thus becoming famous for his ability in writing, was requested to compose an apology for the unfortunate affair at* Preston-Pans *in* Scotland. *This was prefixt as a preface before the report*‖ *that was*

* Encyclopédie, tom. vi. p. 750.

† Journal des Sçavans, Octob. 1757.

‡ Jan. 1758.

§ Those of Mess. Frazier and Losier-Bouvet.

‖ This is intituled, *The Report of the Proceedings and Opinion of the Board of General Officers on their Examination into*

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was published of that transaction, and this preface is esteemed a master-piece in its kind. For there the movements of the English *and of the* Highland *armies are described, the picture of the country drawn, and the advantages and disadvantages of ground pointed out, with such force and perspicuity, that I have heard good judges say, they looked on it, as one of the best written pieces in our language. And indeed its author was very capable of adapting his style to variety of subjects, whether grave or of a joyous nature: so that his immature death has deprived us of many excellent works, he might have otherwise published of very different kinds.*

Henceforward Mr. Robins *had opportunities of making farther experiments in gunnery, by the favour of Lord* Anson; *the result of some of them is declared in the Discourses, I here publish from the author's papers.*

He also not a little contributed to the improvements made in the Royal Observatory at Greenwich, *by using his interest with the same noble person to procure a second mural quadrant, and other instruments, whereby this observatory is become perhaps the compleatest of any in the world. The new mural quadrant, which was at first designed for observing towards the north, being still of more exquisite workmanship than the former, is accommodated for observations towards the south. This observatory soon became famous, from Mr.* Flamsteed's *observations being found to be the most accurate of any made at that time by others. And what may we not expect from it now, since its present worthy possessor, the reverend Dr.* Bradley, *is not more remarkable for his singular exactness in observing, than for his great humanity in communicating his observations to such as are likely to make a proper use of them?*

Mr. Robins's *reputation being now arrived at its full height, neither his backwardness in pushing his for-*

the conduct of Lieutenant-General Sir John Cope, *Knight of the* Bath, *Colonel* Peregrine Lascelles, *&c.* London printed in 1749.

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tune, that conſtantly accompanied him, nor his inflexible honeſty, that never permitted him to approve of the unwarrantable actions of any faction, being no longer able to prevent his preferment, he was offered the choice of two very conſiderable employments. The firſt was to go to Paris *as one of the commiſſaries for adjuſting our limits in* Acadia; *the other to be engineer general to the* East-India *company; whoſe forts being in a moſt ruinous condition, there was wanting a very capable perſon to put them into a proper poſture of defence. This latter he accepted, as it was ſuitable to his genius, and where, he believed, he ſhould be able to do real ſervice, as not being liable to be hindered through the ſuggeſtions of deſign or ignorance, which by their boaſting and importunity, often inſinuating themſelves into the direction of public affairs, frequently render abortive the beſt concerted ſchemes.*

The company's terms were both advantageous and honourable. There was ſettled upon him five hundred pounds per annum during his life; on condition that he continued in their ſervice five years. He was alſo entruſted with the appointment of all that were to be employed under him; and ſuch an order was made for furniſhing him with what ſums of money, he ſhould think neceſſary towards carrying on the works he undertook, as was never paſſed on the like occaſion; ſo great was the confidence the company repoſed in Mr. Robins's *integrity, as well as ability; in neither of which did they find themſelves deceived; and indeed he acted in all occurrences, through every ſcene of life, with the utmoſt generoſity and diſintereſtedneſs; and never offered to undertake any thing, whereof he was not a perfect maſter.*

He deſigned, if he had remained in England, *to have compoſed a ſecond part of the voyage round the world; as appears from the following letter, which Lord* Anson *did Mr.* Robins *the honour to write to him on that ſubject.*

DEAR

DEAR SIR,

When I last saw you in town, I forgot to ask you, whether you intended to publish the second volume of my voyage before you leave us, which, I confess, I am very sorry for. If you should have laid aside all thoughts of favouring the world with more of your works, it will be much disappointed, and no one in it more than

your very much obliged
humble servant,
ANSON.

Bath, the 22d of *October*, 1749.

If you can tell the time of your departure, let me know it.

The above letter is printed not without the noble Lord's conſent; who, being requeſted to permit, that this teſtimony might be exhibited to the world of his Lordſhip's eſteem for Mr. Robins, *replied, in the politeſt manner,* That every thing in his power was due to the memory of one, who had deserved so well of the public.

Mr. Robins *was alſo preparing an enlarged edition of his* New Principles of Gunnery, *and as there would have been made great improvements in what was already publiſhed; ſo the geometrical part was intended to be added; as I learn from ſome memorandums, he left behind him. From them alſo I underſtand, that he had the theory of the moon under conſideration.*

But, having provided himſelf with a complete ſet of aſtronomical and other inſtruments, for making obſervations and experiments in the Indies, *he departed from hence at* Christmas *in the year* 1749, *to the great ſorrow of all his acquaintance. This however was in ſome meaſure alleviated, on account of the benefits the public might receive from his preſent ſituation*

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in life, and by the hopes of seeing him return safe with honour to his native country. Not less sensibly moved was Mr. Robins *at quitting the agreeable society of his dear friends, to many of whom he had been strictly united from his first coming to this place.*

In the voyage his ship was very near being cast away; but he arrived at the Indies *on the thirteenth of* July, 1750.

There he immediately set about his proper business with unwearied diligence; and he completely formed plans for Fort St. David *and* Madras. *But he lived not to put these in execution. For the great difference in the climate was beyond his constitution to support: which was always delicate, though till then he scarce ever had a fit of sickness.*

In September 1750, *he was attacked by a fever, out of which he recovered; but about eight months after he fell into a languishing condition, in which state he continued to his death. When he had reason to believe that was not far off, he expressed himself displeased, the physicians had not made him acquainted with his real case sooner, that he might have lost no time in expectation of recovering; and even then he exerted himself as much as possible in the duty of his office, expiring at* Fort St. David *the 29th of* July 1751, *with his pen in his hand, as he was drawing up for the company an account of the posture of their affairs.*

The fortifications of Fort St. David *have been since finished, and they are at work upon those of* Madras, *according to Mr.* Robins's *plans. These I have heard highly praised by many intelligent persons, who have been upon the spot. And what is still more, I have been informed,* that they were approved of by the brave colonel* Clive; *who through the force of genius alone*

* By the late accurate and elegant historian of the affairs of India, ROBERT ORME, Esq. who was personally acquainted with Mr. Robins, and highly esteemed his abilities. See his Military Transactions of the British Nation in Indostan, 4to.

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alone becoming a self-taught commander, has with matchleſs conduct, as well as valour, retrieved our ſinking affairs in thoſe parts of the world.

As ſoon as the news of Mr. Robins's *death arrived here, great numbers, beſides his particular friends and acquaintance, ſtrongly expreſſed their concern at the loſs of ſo valuable a perſon; and as his letter was read to the court of the* East-India *directors, a moſt viſible appearance of ſorrow ſhewed itſelf on the countenance of all preſent; and the regard the company had for Mr.* Robins's *merit, and their ſenſe of the ſervices he did them, the ſhort time he lived in their employments, induced them to behave with great generoſity towards his father.*

Mr. Robins *left alſo a moſt amiable character behind him in the* Indies; *for I have learnt from many perſons, who have come from thence, that his memory is ſtill held there in the higheſt eſtimation by all ranks of people. And no wonder that a perſon ſo well qualified in every branch of valuable learning, ſuch a proficient both in the practice and theory of uſeful arts, as likewiſe ſo capable and ready to communicate his knowledge to others, and endowed with a moſt candid, generous and diſintereſted mind, and withal a moſt ſprightly and agreeable companion, ſhould have rendered himſelf univerſally acceptable.*

I muſt not omit mentioning, that he took care to make a ſufficient proviſion for his father, Mr. John Robins, *by purchaſing an annuity for the old gentleman's life; who, at the* Bath *till his ninety-ſecond year, when he died in* 1758, *enjoyed a perfect ſtate of health, having had nothing ſo much to regret, as the loſs of the only child he ever had; whoſe reputation in the world, and conſtant affectionate behaviour towards him, were the chief conſolation of his declining age.*

By his laſt will, Mr. Robins *left the publiſhing his works to his honoured and intimate friend* Martin Folkes, *Eſq. preſident of the Royal Society, and to myſelf: but as that excellent perſon had for ſome time*

been

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been rendered incapable, by a paralytic diſorder, of which he is ſince dead, of bearing a part in this charge; Mr. Robins's *papers were entruſted to me by his executor Mr.* Thomas Lewis.

And here I cannot forbear expreſſing my deep ſenſe of the long continued and uninterrupted beneficence, Mr. Robins *experienced from this worthy gentleman, ariſing ſolely from his juſt diſcernment of merit, which not only inſpired him with the higheſt eſteem for Mr.* Robins, *but alſo with a degree of friendſhip, which neither time nor place could abate: his regard for the memory of his deceaſed friend, extending beyond the grave, was ſtill ſhewn in a conſtant benevolence towards Mr.* Robins's *father during his life.*

What writings Mr. Robins *left behind him, that were delivered to me, had been tranſcribed into one volume in a very fair hand. This contained ſeparate diſcourſes, all relating to gunnery.*

Theſe take up from p. 175 *to p.* 315. *They had been moſt of them read before the* Royal Society, *whilſt their author was in* England. *That entitled,* A Comparison of the experimental Ranges of Cannon, *&c. at p.* 230, *was ſent by him from* Fort St. David, *juſt after he was recovered out of his firſt illneſs. It was preſented to the Society the* 27*th of* June 1751; *and the letter, that accompanied it, was dated* October 16, 1750; *in which Mr.* Robins *tells the preſident,* This I here send, with another, I intend by the next spring (when I hope to let you have the complete copy) will finish all I now purpose to publish upon the subject of gunnery, leaving the further completion of it to my future labours, either in this country or in *Europe. The other diſcourſe mentioned in the letter, I take to be* The practical Maxims, *and the complete copy to be the* Manuscript *put into my hands, as far as Mr.* Robins *had finiſhed it: which, I believe, would have been more complete, if he had ſurvived any time longer: for as there are added ſome notes, ſo there were vacant*

ſpaces

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ſpaces left, in order, as it ſhould ſeem, to receive more notes; but death put a ſtop to whatever he then farther intended.

Of the Practical Maxims, *which were not read before the Royal Society, there are many copies abroad; and I collated mine with two others: one my friend Mr.* Glover *furniſhed me with; the other Mr.* Nourse, *who had it ſome years ago of Mr.* Muller. *The varieties in the reading, I found to be very inconſiderable.*

This volume contains likewiſe Mr. Robins's New Principles of Gunnery, *as he printed it (for it is not known, what is become of the many improvements, he had made in it) with the account, that was given of it in the* Philosophical Transactions. *And I have added an extract or two from his looſe papers, as alſo his diſcourſe on the height to which rockets aſcend, together with another on the ſame ſubject, written by his ingenious friend Mr.* John Ellicott, *fellow of the Royal Society, and clock-maker to his majeſty; a perſon of ſtrict honour, whoſe inventions, in mechanics, and reputation for exquiſite workmanſhip, in all ſorts of movements for regulating time, are juſtly celebrated, as well in foreign parts as at home.*

Mr. Robins's *diſcourſe of the nature and advantage of rifled barrel pieces, not having been communicated to me, till almoſt all this volume was printed off; I was obliged to give it there at the end, though ſomewhat out of its proper place.*

All the diſcourſes, that had been read before the Royal Society, were to have been collected, and dedicated by their author to the late prince of Orange; *who had expreſſed the higheſt eſteem for Mr.* Robins, *and his writings, as I learn by a letter of a noble perſon, dated the* 13th *of* August, 1747, *written from the* Hague *to Lord* Anson.

I mentioned before Mr. Robins *having treated of the geometrical part of his* New Principles of Gunnery; *ſo he had conſidered the effects of the air's reſiſtance*

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ſiſtance in regard to ſhip-building, and the working of veſſels at ſea; but whatever he might have ſet down in writing concerning theſe, and other curious and uſeful ſubjects, he carried along with him to the Indies; *of which I have not been able, during the many years ſince his deceaſe, to get the leaſt information.*

JAMES WILSON.

London, May 20,
1761.

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NEW

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NEW

PRINCIPLES

OF

GUNNERY.

CONTAINING

THE DETERMINATION OF

THE FORCE OF GUNPOWDER,

AND

AN INVESTIGATION OF THE DIFFERENCE

IN

THE RESISTING POWER OF THE AIR

TO

SWIFT AND SLOW MOTIONS.

FIRST PRINTED IN 1742.

A

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THE

AUTHOR'S PREFACE.

ABOUT a twelvemonth since, I had some intentions of exhibiting a public course of Fortification and Gunnery: and though, for reasons not necessary to be here mentioned*, I afterwards desisted from that design, yet, as I had proceeded so far as to distribute some manuscript copies of the particulars, of which I proposed it to consist, I have thereby been in some measure engaged in the present undertaking.

For, as I had resolved to render this course as complete as I possibly could, both by large models of different fronts of Fortification, and their different attacks, and by an experimental exemplification

* This sentence seems to allude to a circumstance not noticed in the preceding account of the author's life; viz. that on the establishment of the Royal Military Academy at Woolwich, about that time, Mr. Robins was a candidate for the office of professor of fortification in it, but which was conferred on Mr. Muller. On which, it has been said that Mr. Robins published his discoveries and improvements in Gunnery, to show, it was thought, what sort of a man had been overlooked on that occasion. H.

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plification of the precepts of Gunnery with real Artillery, I found it necessary to insert under this last head a theory of the force of Gunpowder, and certain propositions relating to the resistance of the air, which I had discovered, and confirmed by experiments. But these principles being set down in the schemes, which I delivered out as assertions only, without any account of the nature of the experiments made use of for proving them, and being liable to great contestation, on account of their inconsistency with all the received opinions of the writers upon this subject, I thought it incumbent on me to clear up in a more particular manner any difficulties which might have arisen about them, and to evince their certainty by a number of unquestioned experiments. And this has principally given rise to the ensuing treatise, in which the force and varied action of Powder is so far determined, that the velocities of all kinds of bullets impelled by its explosion may be thence computed, and the enormous resistance of the air to swift motions (much beyond what any former theories have assigned) is likewise ascertained. And on these principles it will appear that the original velocities of bullets, when impelled by full charges of Powder,

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der, and the track described by their flight, are extremely different from what the writers on these subjects have hitherto supposed.

As the principal disquisitions of the following sheets relate to the force of Powder, and the flight of shells and bullets, it may not, perhaps, be unacceptable to the reader to peruse a few particulars relating to the invention of Powder, and the history and improvements of Gunnery, and its sister art, Fortification; especially as the nature and purport of what we shall hereafter advance will receive some kind of illustration, by being compared with the opinions which have formerly prevailed in these enquiries. And though our immediate view is the promoting the theory and practice of Gunnery, yet the present methods of fortifying are so connected with the invention and management of Artillery, (these arts having in some measure given laws to each other) that I presume a short recital of the rise and changes of the modern military architecture will not be impertinently prefixed to an account of those powerful machines which first gave it birth.

With regard to the first invention of Bastions,

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there are many opinions amongst authors, it being as yet a point undecided in what place and at what time they were first put in practice. Some have attributed this invention to *Zisca*, the *Bohemian*; others to *Achmet Bashaw*, who having taken *Otranto* in the year 1480, fortified it in a particular manner, which is supposed to be the first instance of the use of Bastions*. But these are the positions of later writers. Those who wrote on the subject of fortification near two centuries ago, seem to suppose that Bastions were a gradual improvement in the ancient method of building, rather than a new thought, that any one person could claim the honour of.—*Pasino* in particular, in the first part of his book, imputes the changes in the ancient fortifications, and the introduction of the modern form, to the increased violence of the later artillery, without pretending that it was effected at one time, or by one person †. So that I believe we cannot with certainty

* *Vid.* The commentary of the Chevalier Folard on Polybius. Tom 3. pag. 2.

† Vid. *Discours sur plusieurs poincts de l'Architecture de Guerre concernants les Fortifications, tant anciennes que modernes*, &c. *Par M.* Aurelio de Pasino Ferrarois, *architecte de tres-illustre Seigneur, Monseigneur le Duc de* Bouillon.—Printed by Plantin 1579. It appears by a copy of verses prefixed to the book, that this author fortified Sedan.

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certainty affirm more in relation to the invention of Bastions, than that they were well known soon after the year 1500. For in 1546, *Tartalea* published his *Quesiti & inventioni diverse*, in the sixth book of which he mentions, that whilst he resided at *Verona* (which must have been many years before) he saw Bastions of a prodigious size, some finished, and others building; and there is besides, in the same book, a plan of *Turin*, which was then fortified with four Bastions, and seems to have been completed some time before.

And though we cannot certainly assign the time when the old circular Towers were first converted into Bastions, yet in all probability it did not long precede the date we last mentioned. For in the same book, the prior of *Barleta*, who was himself a soldier, esteems *Turin* to be impregnable, and tells us that this was the general opinion of all men of skill; he likewise makes it a question, if, in the fortifying of cities, the genius of mankind was not arrived at its utmost limits of perfection; which seems to evince that the invention was a recent one, and that it was greatly the object of the esteem and considera-

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tion of his cotemporaries, as a new contrivance of this kind would naturally be.

The first Bastions, such as those of *Turin*, of *Antwerp**, and others of the same age, were but small, and removed at a great distance from each other; for at that time it was the universal practice to attack the Curtain, and not the Bastions. But a few years after there were introduced Bastions much larger, and much nearer together, than what had been constructed before; as appears by the citadel of *Antwerp*, which was built under the direction of the duke *D'Alva*, about 1566, and which, by the frequent encomiums on it in some early authors, seems to have been the first instance of this improvement.

From this period, the modern practice of military architecture may be supposed to have taken its rise; most of the improvements of the present times being little more than the putting in use such methods as were proposed within a few years of this æra; for many celebrated authors flourished soon after, as †*La Treille*, *Alghisi*, *Marchi*,

* *Antwerp* was fortified about the year 1540, as we learn from Speckle. Lib. I. chap. 10.

† Vid. La maniere de fortifier Villes, Chasteaux, et faire autres

Marchi, Pasino, and, above all, *Speckle**, who was one of the greatest geniuses that has applied to this art.

The better to judge of the pretensions of the moderns, and the merit of the systems of fortification now in vogue, we must enter into a short discussion of the various methods which have been proposed for covering the Flanks, and consequently for securing the Ramparts from the approach of an enemy. For if it be agreed that the principal defence of a fortress is its Flanks, the best standard to judge of the merit of any system of fortification, is the manner in which it provides for the safety of the Flanks, against the efforts of the enemy.

Now the most usual contrivances for this purpose have been Orillons, Ravelins placed before the Curtains, Half-Moons placed before the points

autres lieux forts: mis en François par le Seigneur *de Bereil* François de la Treille Commissaire en l'Artillerie. A *Lyon* 1556. This author was the first I have seen who proposed the Retired Curtain, which has since been published by others under the name of the Re-inforced Order.

* *Daniel Speckle* was architect of the city of *Strasbourg*; he died in the year 1589. He published a treatise of fortification in *German*, which was re-printed at *Leipsic* in the year 1736.

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points of the Bastions, and Contregards; each of which we shall separately consider, both as to their use and antiquity.

The Orillon is as old as the Bastion, since in *Turin* and *Antwerp* (mentioned above) there is a lower Flank, which is cut out of the substance of the Bastion, and has thereby a shoulder of a considerable thickness, to screen it from the Field-Batteries. But, besides this, the drawings of *Pasino*, *Speckle*, &c. abound with Orillons of the same form with those now used, the only difference being, that the modern ones are less massive than the ancient ones. This invention has had the good fortune to stand its ground in almost every system which has prevailed, although it be rather on the fame of the services it has formerly done, than for any advantages the moderns have received from it. For in ancient sieges it was the custom for the besieged to have a retrenchment behind the breach, by which means the besiegers were obliged to lodge themselves on the ruins of the breach, in order from thence to batter the retrenchment. In this case the piece or pieces of artillery, which being covered by the Orillon, could not be dismounted, were of wonderful

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derful service to the besieged; and many instances might be given, where the enemy have been hereby so gauled, after they had lodged themselves in the ruins of the breach, that they have desisted from their enterprize. But as it is now no longer the fashion to hold out after a breach is made in the body of the place, and the ditch is near filled up, we rarely hear in the present times of any great feats performed by the Orillon.

The Ravelins placed before the Curtains, (or Half-Moons as they are called in the modern systems) were intended to protect the Flanks from cross shot, and to confine the batteries, which should be raised against the Flanks to the opposite part of the Counterscarp only, where they would be more exposed to the besieged, and more difficult to preserve. This invention likewise is nearly as ancient as the art of fortification, it being to be found in great numbers of old places, and in almost every old writer, and is still continued in most fortifications.

But the ancient writers, whose principal care was the securing of their Flanks, did not rely solely on the advantages they received from the last-

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last-mentioned invention. For though, by that means, the Batteries for destroying the Flank were confined to one place, yet they found, on examination, that on that place the enemy would have more room than was sufficient for erecting of his Counter-Batteries, and therefore they added Half-Moons before the points of the Bastions: these were intended to possess the ground to which the enemy's Batteries against the Flanks were already confined, and thereby to render the construction of those Batteries still more difficult. However, they did not completely answer this purpose, and have been long since laid aside.

The intention of Contregards*, which are likewise very ancient, is the same with that of the Half-Moons last-mentioned; that is, the protection of the Flanks, to which purpose (if properly constructed) they are most wonderfully adapted; for the enemy, in order to ruin the Flank, must either plant his Counter-Battery on the Contregard itself, which, if the Contregard be of a proper profile, it will be impossible for him to do, or he must demolish a part of the Contregard

* *Pasino*, whom we have mentioned above, claims the invention of Contregards, though they were afterwards much mended by *Speckle*. But the Contregards of this author were not before the Bastions only, but surrounded the whole place.

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Contregard to enable his battery on the Contrescarp to view the Flank, which is a tedious work, attended with great hazard and difficulty. The same inconveniency likewise attends him, when he would batter in breach.

But, notwithstanding the excellence of this invention, it has been almost entirely neglected in the modern system of a neighbouring nation.—There have indeed been two or three places fortified by the *French*, in which there are pieces called by them Contregards, but they have nothing but the name in common with those we here treat of. However, their experience of the efficacy of this work at *Turin* may possibly have induced them to think more favourably of it: for I have lately seen them adding Contregards to the old works of a very considerable frontier, although it was before esteemed one of their completest places.

From all that we have said, then, it appears that the covering of the Flanks was a subject much more attended to by the ancient engineers, than by those who have succeeded them; and consequently that the art of Fortification has not received

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ceived from the moderns those great improvements which unskilful writers would sometimes persuade us to believe: for, indeed, in the securing of the Flanks consists the greatest strength of a fortress; since, though all the other defences, by being exposed to the Field-Batteries of the enemy, should be ruined, yet, as long as the Flanks are entire, the Rampart of the place cannot be approached by the enemy: and, therefore, since this circumstance hath been so little heeded by some amongst the moderns, it must be owned, that the true principles of this art have been very imperfectly comprehended by them.—For it has often happened, that they have disputed about a few fathoms in the length of a Flank, a Face, or a Curtain, or a few degrees in the magnitude of a particular Angle; when at the same time they have too much disregarded this most important consideration of all, the screening of the Flanks from the Batteries of the enemy.

But this neglect hath been sometimes owing to the authority of erroneous maxims, one of which, in particular, is, that* whatever sees, is itself seen;

* See this maxim urged with this view in Pagan's Fortification, Chap. iv.

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seen; whence it has been inferred, that, if the Flank can see the enemy, the enemy can ruin the Flank with his batteries. But the fallacy of this reasoning lies here, that the Flank, if properly covered, cannot see the enemy when he is in a situation where it is possible for him to raise batteries, but only when he gets in a place where he must be exposed to the fire of the Flank, without having it in his power to return it. For instance, a piece of cannon, covered by an Orillon in the common manner, cannot be seen by the enemy, till he is got over the greatest part of the ditch, or is mounting the breach, in either of which places it is impossible for him to raise a counter-battery: and the more complete the artifice is by which the Flank is screened, the greater will be the space in which the enemy will be thus exposed.

Other engineers have endeavoured to undervalue this art as ineffectual, and with this view they have expatiated much on the force of the modern methods of attack, and have declared, that no place, how artfully soever constructed, can stand before them. With these gentlemen it is a maxim, that when the Contrescarp is once lost, the whole contest

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contest is in a manner over, and they endeavour to support themselves in this persuasion by the examples of places of great note, which have been reduced in a much shorter time than was expected. If these opinions could be relied on, the greatest part of the money laid out in fortifications would be extremely ill employed; since a simple Rampart and a Contrescarp would fully answer the whole purpose intended. But the truth is, that when a place is well constructed, and skilfully defended, the taking of the Contrescarp is but a small step towards the possession of the place*. Indeed the rashness and precipitancy of the director of the approaches hath often intimidated a weak and ignorant governor; but when the attacks have been thus eagerly hurried on against a place commanded by a brave and knowing officer, he has sometimes taken such advantages of these incautious steps, as have made them too fatal to be copied by any pretending to prudence or humanity. By this means the easiest enterprizes have been often rendered impossible, and

* In the last memorable siege of *Barcelona*, the loss of the Contrescarp (which was taken in a fortnight) did not determine the fate of the town, the great resistance being after the body of the place was opened by several breaches.

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and the pretence of gaining a day or two has often occasioned the loss of the whole*.

Besides those inventions for screening of the Flanks, which we have already mentioned, there have been others proposed of a different nature, which, by reason of their singularity, have been less attended to; such is the constructing of a Line, which should pass through the ditch, from the point of the Bastion to the opposite point of the Contrescarp. This is mentioned by general *Montecuccoli*, in his Memoirs, as a method much less liable to exception than it appears to be at first sight †. But though a Line thus constructed will doubtless cover the Flanks from the view of the batteries placed on the opposite part of the Contrescarp, and is itself very defensible; yet I have never heard of its being put in execution.

Another

* Many instances of the difficulties and hazards to which the allies were often exposed in Flanders, during the late war, may be seen in *Landsberg*, who was then an engineer in the service of the States-General: these accidents, according to him, were generally owing to the presumption of the directors, who, under the pretence of expedition, contracted the front of their attacks, and thereby often left the enemy's works in their rear, which rendered their progress next to impossible.—Vid. *Nouvelle Maniere de Fortifier les Places.*

† Vid. Memorie del general Principe di Montecuccoli, pag. 116.

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Another way of securing the Flanks, is by interposing the Entring Angle of the Contrescarp, (or of the Ravelin) between them and the counter-batteries. This practice is described by *Errard* of *Barleduc**, and is by him said to be the invention of the count of *Lynar*. And though some authors, who were ignorant of the advantages hereby proposed, have severely censured the having any part of the ditch hidden from the Flank, a circumstance which must necessarily attend this construction; yet the greatest genius who ever applied himself to the study of this art has thought it worthy of his imitation, the celebrated fortress of *Berghen-Op-Zoom* having its Flanks in part covered by this artifice.

But in a proper soil there is still a more efficacious defence than any we have yet mentioned; and that is by the means of Contremines. For, supposing the fortifications of a place to be constructed with no more art than what is necessary to

* Vid. *La Fortification demontrée*, Lib. iii. Chap. ii. Besides this invention here mentioned, there occurs in this author, the contrivance of placing a Gallery under the Covered-Way, with loop-holes into the ditch, which is practised at *Tournay*, but more completely at *Berghen-Op-Zoom*. Vid. Lib. iv. Chap. vii.

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to oblige an enemy to bring his batteries on the Glacis, when he proposes either to batter in breach, or to ruin the Flanks (which may be effected by a good Profile and a Ravelin before the Curtain only.) If the soil be free from water t. a considerable depth, it is always in the power of the besieged to ruin the batteries of the ehemy by their Mines, which may be repeated too a number of times, in proportion to the depth of the soil: for these batteries being by supposition confined to one situation, the besieged can always be prepared for these operations before-hand, and would have infinite advantages over an enemy who should endeavour to dig them out; which, however, in such circumstances, would be his only resource.

The first successful application of the blowing of Mines in sieges, was in the kingdom of *Naples;* where *Pietro de Navarre,* by this means, possessed himself of a fort garrisoned by the *French.* But the first celebrated use of these Mines, in oppos ing the progress of the besiegers, was in the years 1666, 67, 68, at the siege of *Candia*; not but they had been often practised in the defence of places before, though in a less memorable man-

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ner; for, by the assistance of this invention principally, the city of *Candia* kept the whole power of the *Ottoman* empire at a bay for three years successively. Since that time the advantages of Contremines have been better understood. The last eminent instance of their great usefulness was in the defence of *Turin*, in the year 1706; for so effectually were the besiegers traversed thereby, that, after near four months of open trenches, they were not in the possession of more than the Contrescarp, and even there, eleven pieces of their cannon were blown up by the defendants, but three or four days before the place was relieved.

Before I leave this head, I cannot but in justice mention the great improvement in the doctrine of Mines, which is contained in that excellent Dissertation* annexed to the third volume of the *French Polybius*. For nothing can be more complete than the manner in which the different stages of Mines are there distributed; indeed the form there assigned to the excavation cannot be rigorously

* This, in the preface, is said to be the performance of monsieur *de Valiere*, Marechal des Camps, and captain-general of the miners.

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rigorously what the author seems to suppose; but this exception has nothing to do with his general rangement of the Chambers, which is extremely well contrived for the husbanding of the ground, and the annoyance of the enemy.

I have already taken notice of the defects in the writings of many of those who, amongst the moderns, have undertaken to form systems of fortification. But when I speak of these authors and their copiers, I must at the same time avow the superior merit of the great *Coehoorn*, who was undoubtedly the ablest fortifier that ever the world knew. This author has published two treatises on this subject; the first containing a method of fortifying a Pentagon, to which is annexed a project for the amending the fortifications of *Coevoerden*. In his second, he has proposed three different manners of fortifying—one applied to a Hexagon, another to a Heptagon, and a third to an Octagon; and he has besides added the manner of fortifying that side of a fortress which happens to be contiguous to a river. In this work he has particularly examined all the possible attacks that can be formed against his proposed places, thereby to evince the great su-

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periority of his defences; so that it is in some measure a discourse on the attack and defence of places, as well as a system of fortification; and, upon the whole, is the most excellent performance that has ever been produced on this subject. It was written in *Low-Dutch*, (the author's native language) but has been translated into both *French* and *English*, but very imperfectly; though in a new edition of the *French* translation, lately printed in *Holland*, many of the errors of the former are amended, and some particular passages are cleared up by the notes of the editor, who seems to have understood his author very well.

I have been told by those who were well acquainted with this great man, that his treatises were far from acquiring him either the advantages or reputation which he might reasonably have hoped from them; for that his cotemporary engineers, wedded to their old road, decried him as an unskilful, self-conceited pretender; but that he at last surmounted these effects of their envy and prejudice, by his defence of fort *William* at *Namur*, when that place was besieged by the *French*; after this, which established his reputation, he rose apace to the greatest military commands, and

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and immortalized his name by his conduct of the siege of *Namur*, under King *William*, and afterwards at *Bon*, *Limburg*, the citadel of *Liege*, *&c.* And his death at the beginning of the late war in *Flanders* was a very great misfortune to the allies; of which almost every siege formed by them after the year 1707 was a melancholy proof.

Besides being entrusted with the direction of sieges, he was employed too in the repairing and new-modelling many of the *Dutch* frontiers.—His last work, which is left unfinished, was *Berghen-Op-Zoom*, which will always do honour to his memory. Though he is yet so little out of the reach of censure, that I have heard military men, even in that place, condemn, as imperfections, those very circumstances whence it derives its principal defence.

Considering the great fame which general *Coehoorn* acquired in real service, it is difficult to account for the little regard which hath been paid to his writings. The most natural reason I can discover for this negligence, is the proneness which we have always shewn for the opinions of a neighbouring nation, who, whatever other good

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qualities they may have, were never famous for doing justice to the merit of those who were of another country, and were engaged in an interest opposite to their own. However, I presume, his reputation as an author is at present increasing. For I saw, not long since, in one of the most considerable frontiers belonging to France, a piece of fortification carrying on, which was evidently copied from the printed works of *Coehoorn*.

Though, with regard to the modern writers on fortification, I cannot find another to place in the same article with the great genius last mentioned, yet there are two authors on the methods of attacking and defending places, (a subject nearly connected with fortification) who merit the highest applause:—I mean *Goulon* and the Marechal *de Vauban*. The first, in a short treatise, entitled *Memoires sur l'Attaque et la Defence des places*, in which he has very distinctly inculcated the principal maxims necessary in those operations. The other, in a work which he presented in manuscript to the late King of *France*, of which copies getting abroad, it was published four years since in *Holland*. In this book Mr. *Vauban* has very circumstantially described those parts of the attack,

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attack, which were more immediately of his own invention; such as the *Batteries à Recochet*, the Parallels, and a peculiar conduct of the Sap. Not but that he has likewise given very ample instructions on every other necessary head; and the whole must be owned to be a very masterly performance, worthy of the experience and capacity of its great author.

It might, perhaps, be expected that I should here mention with approbation the skill of this last-mentioned engineer in the art of fortifying. But as he has never written any thing himself on this subject, that may excuse me from ranging him in the list of authors. But to speak the truth on this head, I cannot but believe, from all I have hitherto seen of his works, that he was much more to be esteemed for his other talents, than for the fortifications he has erected. For though I have a very high opinion of his good sense and discernment, I do not conceive that his invention in this art was to be compared with that of his cotemporary *Coehoorn*.

Thus much may suffice on the origin and variations of the present military architecture. We must

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must next discuss what is more immediately connected with the purport of the ensuing treatise; I mean the invention of Powder and Artillery, with their respective improvements, and the different theories they have given rise to.

The invention of Gunpowder is usually ascribed to one *Bartholdus Schwartz*, a *German* Monk, who discovered it, as is said, about the year 1320; and the first use of it in war is commonly supposed to have been by the *Venetians* against the *Genoese* about the year 1380. But both these suppositions are undoubtedly false; for a composition resembling that which we call Powder is mentioned by *Roger Bacon*, as well known in his time, and he lived near fifty years before *Schwartz*; and there are indisputably proofs of the use of Artillery much earlier than the year 1380.

Indeed, as the time of the discovery of saltpetre is confessedly uncertain, it is not to be wondered at, that the invention of Gunpowder should be obscure and unknown; for these two discoveries are so connected, that it is difficult to conceive how the first could be long known before the latter was found out.

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The distinguishing property of saltpetre is the prodigious increase of inflammability which it produces in all burning substances, when mixed with them; although alone and unmixed it will neither flame nor burn. For instance, saltpetre put into a crucible, and placed in the hottest fire, will only melt, and grow red hot, but will neither explode nor flame; yet if any inflammable substance (sulphur, suppose, or coals) be thrown into it, a violent blaze will be instantly produced, in which a part of the saltpetre will be consumed in proportion to the quantity of the inflammable substance which was put to it; and a like explosion will take place if saltpetre be thrown upon any fire. Now it cannot be reasonably supposed that this quality of saltpetre could be long unknown after the substance itself was discovered; for the accidental dropping of any small part of it into the fire, would prove its prodigious explosive power when mixed with burning bodies.—And this being once observed, it was thence a very natural and obvious thought to invent a composition of saltpetre mixed with any inflammable substance which would burn more violently than any known before: and our present Gunpowder

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powder is only the improvement and perfection of such a mixture.

On this supposition, then, if we knew the time when saltpetre first came in use, we might give some guess when mixtures resembling our present gunpowder were first invented. Now the most general opinion on this head is, that saltpetre was first discovered either by the *Arabians* or the later *Greeks*, about the middle ages of our æra, when alchymy and chymistry were eagerly pursued by both nations; for its *Arabic* name is said to be expressive of its explosive quality; and the *Greek* fires used in war by the later *Greek* Emperors (if the effects attributed to them by many authors are true) must have had saltpetre in their composition.

Indeed some moderns (misled by a similarity of name) have supposed saltpetre or nitre to have been known to the ancients. But chymists are now agreed, I think, that the substance mentioned by some ancient writers, and described by *Pliny*, by the appellation of nitre, is a salt altogether different from what we call saltpetre.

Now,

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Now, that the first invention of Gunpowder (or of compositions resembling it) did long precede the time of *Schwartz* or of *Bacon*, and may thence be reasonably supposed nearly coeval with the knowledge of saltpetre, appears from *Bacon** himself; for it is not a new composition which he proposes, but the application of an old one to military purposes. And from his words it plainly appears that a mixture of saltpetre with other substances was then vulgarly used for the making of recreative fire-works. And this appears yet plainer from the treatise of *Marcus Græcus*, entitled *Liber ignium*†; for this author describes two

* *Bacon* tells us, that sound like thunder, and lightnings greater than those produced by nature, might be made by art; and this many ways, by which a city or an army might be destroyed; and he supposes it to be by an artifice of this kind that *Gideon* defeated the *Midianites*; and having in another treatise mentioned almost the same thing in different words, he adds, *Et experimentum hujus rei capimus ex hoc ludicro puerili quod fit in multis mundi partibus. scil. ut instrumento facto ad quantitatem pollicis humani ex violentia illius salis, qui* SAL PETRÆ *vocatur, tam horribilis sonus nascitur in ruptura tam modicæ rei, scil. modici pergameni, quod fortis tonitrui excedere rugitum, & corruscationem maximam sui luminis jubar excedit. Vid.* Doctor *Jebb's* Preface to his edition of *Bacon's Opus Majus*.

† This is a manuscript in the possession of Dr. *Mead;* but what is here mentioned is quoted by the editor of *Bacon's Opus Majus* in the preface.

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two kinds of fire-works, one for flying, and the other for making a report. The case or *Cartouche (Tunica)* for the first he directs to be made long and slender, and the composition to be very close rammed; the case for the second he orders to be thick and short, to be strongly tied at both ends, and to be but half-filled; and the composition he prescribes for both is two pound of charcoal, one pound of sulphur, and six pound of saltpetre, well powdered and mixed together in a stone mortar: and this will be allowed to be a stronger composition than what great quantities of Powder are every day made with. Now though the age of this writer is not well ascertained, yet it must have preceded the use of artillery; for he does not in any place (as I can learn) mention these compositions as used in war; and as he pretends not to be the inventor of these serpents or crackers, (for such we should now call them) nor speaks of them as recent, we may reasonably presume they were in use long before his time.

The first application of this mixture to military affairs seems to have been soon after the year 1300. *Bacon's* proposal (which was about the year

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year 1280) to make use of its enormous explosion for the destruction of armies, might give the first hint, which others might afterwards pursue.—*Schwartz*, instead of being the first inventor of Gunpowder, might possibly be one of the first who thus applied it; and, indeed, the common account of the manner in which he came at his invention, very much favours this opinion*: and perhaps the different improvements soon added by others, or the prosecution of *Bacon's* thought in different places, may have given rise to the different dates assigned by historians for the first use of artillery.

Gunpowder,

* The usual manner in which it is told is, that *Schwartz* having pounded the materials of Gunpowder in a mortar, which he afterwards covered with a stone, a spark of fire accidentally flew into the mortar, and the explosion blew the stone which covered the mortar to a considerable distance.—Now we have proved that *Schwartz*, who was a chymist, could not discover the composition by this means, because it was commonly known before; but he might from hence be taught the simplest method of applying it in war: for *Bacon* seems rather to have conceived the manner of using it to be by the actual effort of the flame against the bodies it might meet with in its expansion. The figure and name of mortars given to a species of the old artillery, and their employment, (which was throwing great stone bullets at an elevation) very much corroborate this conjecture.

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Gunpowder, for some time after the invention of artillery, was of a composition much weaker* than what we now use, or than that ancient one mentioned by *Marcus Græcus*; but this, I presume, was owing to the weakness of their first pieces, rather than to the ignorance of a better mixture; for the first pieces of artillery were of a very clumsy, inconvenient make, being usually framed of several pieces of iron fitted together lengthways, and then hooped with iron rings; and as they were first employed in throwing stone bullets of a prodigious weight, in imitation of the ancient machines to which they succeeded, they were of an enormous bore. But the difficulties of conducting and managing these cumbrous pieces, and the discovering that iron bullets of much less weight than stone ones would be more efficacious, if impelled by greater quantities of stronger powder, soon occasioned an alteration in the matter and fabric of these first pieces, and gave rise to what we style brass cannon, which, though lighter

* *Vide Tartalea* in his *Quesiti & Inventioni*, Lib. 3. *Quesito* 5. where there are set down twenty-three different compositions made use of at different times; the first of which, being the most ancient, contains equal parts of nitre, sulphur, and charcoal.

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lighter and more manageable, were yet much stronger in proportion to their bore; by which means they would endure great charges of a better powder than what had first been used; and their iron bullets, (which were from forty to sixty pound weight) being impelled with greater velocities, were more effectual than the weightiest stones could ever prove*.

By

* The time when this change took place, and the advantages arising from it, are mentioned by *Guicciardin*, who, speaking of the *French* army intended for the invasion of *Italy*, in the year 1494, says,

——— *Et per unirsi con questo esercito erano state condotte per mare a* Genoua *quantità grande d'artiglierie da battere le muraglie, & da usare in campagna, ma di tal sorte, che giammai non haveva veduta* Italia *le simiglianti. Questa peste trovata mólt' anni innanzi in* Germania, *fu condotta la prima volta in* Italia *da'* Venetiani *nella guerra, che circa l'anno della salute* 1380, *hebbono i* Genouesi *con loro.* ——— *Il nome delle maggiori era bombarde, le quali, sparsa dopo questa inventione per tutta* Italia, *s'adoperavano nell' oppugnationi delle terre, alcune di ferro, alcune di bronzo, ma grossissime, in modo che per la macchina grande, & per l'imperitia de gli huomini, & mala attitudine de gl' instrumenti tardissimamente & con grandissima difficultà si conducevano, piantavansi alle terre co' medesimi impedimenti, & piantate era dall' un colpo all' altro tanto intervallo, che con piccolissimo frutto, a comparatione di quello, che seguitò dopo, molto tempo consumavano, donde i defensori de' luoghi oppugnati havevano spatio di potere otiosamente fare di*

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By this means, powder compounded in the same manner which is now practised by all *Europe*, came in use*. But the change of the proportion

di dentro ripari & fortificationi ——— *Ma i* Francesi *fabricando pezzi molto più espediti, nè d' altro che di bronzo, i quali chiamavono Cannoni, & usando palle di ferro, dove prima di pietra, & senza comparatione più grosse & di peso gravissimo s'usavano, li conducevano in sulle carette, tirate (non da buoi, come in* Italia *si costumava) ma da cavalli con agalità tale d'uomini, & d'instrumenti deputati a questo servigio, che quasi sempre al pari de gli eserciti caminavano, & condotte alle muraglie erano piantate con prestezza incredibile, et interponendosi dall' un colpo all' altro piccolissimo intervallo di tempo, sì spesso, & con impeto sì gagliardo percuotevano, che quello che prima in* Italia *fare in molti giorni si soleva, da loro in pochissime hore si faceva. Vid.* Guicciardin's History, L. 1. p. 24. 4to. Venet. 1562. What this author observes of the prodigious size of the stone bullets used whilst the old pieces were in fashion, will be better understood by knowing, that when *Mahomet* the second besieged *Constantinople*, in the year 1453, he battered the walls with stone bullets, and his pieces were some of them of the calibre of 1200 pounds; but then they could not be fired more than four times a day.

* We learn from *Tartalea*, that the cannon powder in his time *(polvere grossa moderna)* was made of four parts saltpetre, one part sulphur, and one part charcoal; and the musquet powder of forty-eight parts saltpetre, seven parts sulphur, and eight parts charcoal; or of eighteen parts saltpetre, two parts sulphur, and three parts charcoal. These compositions for musquet powder are very near the present standard; the first having in one hundred pounds of powder about one pound of saltpetre more than is at present allowed, and the second three pounds more.

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portion of the materials composing it, was not the only improvement it received. The invention of graining it is doubtless a considerable advantage to it; for powder at first was always in the form of fine meal, such as it was reduced to by grinding the materials together. And it is doubtful whether the first graining of powder was intended to increase its strength, or only to render it more convenient for the filling into small charges, and the charging of small arms, to which alone it was applied for many years, whilst meal-powder was still made use of in cannon. But at last the additional strength which the grained powder was found to acquire from the free passage of the fire between the grains, occasioned the meal-powder to be entirely laid aside*.

The

* That powder was first used in meal, and that long after the invention of graining it for the use of small arms, cannon-powder continued in its old form, are facts not to be contested. *Tartalea*, in his *Quæsiti*, L. 3, *Ques.* 9 and 10, expressly asserts, that then the cannon-powder was in meal, and the musquet-powder grained. And our countryman, *William Bourne*, in his ART OF SHOOTING IN GREAT ORDNAUNCE, published forty years after *Tartalea*, tells us in chap. 1. that serpentine powder (which he opposes to corn or grained-powder) should be *as fine as sand, and as soft as flour:* and in his third chapter he says that two pounds of corn-powder will go as far as three pounds

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The formation of artillery hath been very little improved in the last two hundred years; the best pieces now cast not differing greatly in their proportions from those made in the time of the emperor *Charles* V. Indeed, lighter and shorter pieces have been often proposed and essayed; but though they have their advantages, and are extremely useful in particular circumstances, yet it seems now to be agreed that they are altogether insufficient for general service*. But though the proportions

pounds of serpentine-powder. Also Sir *Henry Manwayring*, in his SEAMAN'S DICTIONARY, presented to the Duke of *Buckingham* in the time of *Charles* I. under the word *powder*, tells us, *there are two kinds of powder, the one serpentine-powder, which powder is dust (as it were) without corning——The other is corn-powder*; though he informs us the serpentine-powder was not used at sea. Indeed, when that book was wrote, I believe, powder was usually corned, for the foreign writers on artillery had long before recommended its general use.

* Since the time of Mr. Robins, lighter cannons, but of wider bores, and consequently heavier shot, have been much used, especially for the sea service. Indeed Mr. Robins himself, some years after the above was written, proposed to the Admiralty a regulation of that kind, being the 8th paper in this vol. at p. 279, &c. Those new pieces have been introduced chiefly under the denomination of Carronades, a species of ordnance very short, but of a large bore, and having little or no windage. These were at first introduced as pieces possessing some peculiar and mysterious qualities, by which, with little powder, they could throw a very large shot to a great distance.

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proportions of artillery have not been much varied within that period, yet its use and application have undergone considerable changes, the same ends being now generally pursued by smaller pieces than what were formerly thought necessary. Thus the battering-pieces, now universally approved of, are the demi-cannons, carrying a ball of twenty-four pound weight; it being found, by experience, that their stroke, though less violent than that of larger pieces, is yet sufficiently adapted to the strength of the usual profiles of fortification, and that the facility of their carriage and management, and the ammunition they spare, give them great advantages beyond the whole cannons formerly employed in making breaches. The method too (now generally followed) of forming a breach, by first cutting off the whole wall as low as possible, before its upper part is attempted

tance. The fact is, the small degree of windage confines the whole effect of the powder to the ball itself, which is in a great measure lost in the old guns; and the great weight of the shot enables it much better to overcome the resistance of the air, and consequently to range the farther. And the hint of these advantages and constructions was probably taken from experiments made by myself in the year 1775, with Mr. Robins's ballistic pendulum, and which were published in the Philosophical Transactions of the year 1778. H.

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attempted to be beat down, seems also to be a considerable modern improvement in the practical part of artillery: for I do not remember to have seen this procedure recommended by any ancient author; and *Gabriel Busca**, who boasts much of his great experience, expressly directs the contrary. Indeed *Collado* mentions it as the practice of the *Turks*†, but it is without commending it, or proposing it as an example to be followed.

But the most important improvement in the practical

* Vid. His *Instructione de' Bombardieri*, printed at *Carmagnola* in 1584, cap. xxxvii. in which place he orders the breach to be begun at the upper part of the wall, and from thence to be continued downwards.

† Vid. *Pratica Manuale di Artiglieria dal. Mag. Signor Luigi Collado Hispano, Bettico, Nebrisense*, printed at *Venice* in the year 1586, cap. xx. where he says——*nelle fattioni del gran Turco —— sempre si adoperano i pezzi —— da tagliare le muraglie per di sotto di esse transversalmante, et dipoi di alto in basso a perpendicolo, & applicandosi poi tutti a un tratto i basilischi, con che fanno cascar giù quella parte di muraglia che era già tagliata.* This book here quoted was composed and published in *Italian*, although the author was a *Spaniard*. But he served as an engineer in the *Spanish* army in *Italy*. And he tells us in his preface, that he soon intended to re-publish it in *Spanish;* which last edition is, I presume, what is quoted by *Blondel*, in his *Art de jetter les Bombes.*

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practical management of artillery (for of the scientific part we shall treat by itself) is the method of firing with small quantities of powder, and elevating the piece so that the bullet, in its descent, may just go clear of the parapet of the enemy, and drop into their works. By this means, the bullet coming to the ground in a small angle, and with a small velocity, it either bounds or rolls along in the direction it was fired in; and, therefore, if the piece be placed in a line with the battery it is intended to silence, or the front it is to sweep, each shot rakes the whole length of that battery or front, and has thereby infinitely more chance of disabling the defendants, and dismounting their cannon, than it would have, if it was fired against the same works in the common manner. This disposition of artillery, which is indeed a most useful one, is the invention of the Marechal *de Vauban*, and is by him styled *Batterie à ricochet**, and was first put in practice at the siege of *Aeth*, in the year† 1692

After

* Vid. his book *De l'Attaque et la Defence des places.*

† *Vid.* the Journal of this siege, printed at the end of the last edition of *Goulon's Memoirs.*

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After this brief recital of what has been done in the mechanic part of Gunnery, we must next mention the different theories which have been from time to time advanced in relation to the motions of shells and bullets; in which enquiry we shall not, indeed, find many things worthy of approbation, or even of attention; but, however, as it is a theme in some measure connected with the subject of the following treatise, we must beg the reader's indulgence.

The first author I have seen, who has professedly written on the flight of cannon-shot, is *Tartalea*, a celebrated *Italian* mathematician, famous for having invented the method of solving cubic equations, which is usually ascribed to *Cardan*. This author, in his *Nova Scientia*, printed at *Venice* in the year 1537, and afterwards in his *Quesiti et Inventioni diversi*, printed at the same place in 1546, has professedly discussed several particulars relating to the theory of these motions. And though the then imperfect state of mechanics furnished him with very fallacious principles to proceed on, yet he was not altogether unsuccessful in his enquiries; for he is supposed to be the first who asserted that the greatest

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greatest range of projectiles was at an elevation of 45°. He likewise determined (contrary to the opinion of practitioners) that no part of the track described by a bullet was a right line, although the curvature was in some cases so little, as not to be attended to, he comparing it to the surface of the sea, which, though it appears to be a plain, when partially considered, is yet undoubtedly incurvated round the centre of the earth. He also assumes to himself the invention of the gunner's quadrant, and has often given shrewd guesses at the event of some untried methods which were proposed to him. But as he had never been conversant in the practice of artillery, but founded his opinions on speculation only, almost all the writers who succeeded him were perpetually carping at him, though often without naming him; of which many examples might be given from the works of *Busca*, *Collado**, *Ufano*, *Simienowicz*, &c.

* *Collado*, cap. lxii. denies that *Tartalea* was the inventor of the gunner's quadrant, and quotes *Daniel Santbech*, or *Regiomontanus* (for he confounds them) as having known it many years before. But the truth is, that *Santbech's* book, from whence his quotation is taken (*Problematum Astronomicorum & Geometricorum sectiones septem*) was not printed till the year 1561, which was long after *Tartalea*. Nor did *Santbech*, though he talks of the different elevations of artillery, know the method of framing a quadrant proper for his purpose.

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&c. And the philosophers of those times often intervening in the questions hence arising, there were hereby many disputes on motion set on foot, (especially in *Italy*) which continued till the time of *Galileo*, and perhaps gave rise to his celebrated dialogues on motion, which were first printed in the year 1638. And in this interval, or before the doctrine of *Galileo* was established, many theories of the motions of military projectiles, and many tables of their comparative ranges at different elevations, were published; all of them egregiously fallacious, and utterly irreconcileable with the motions of those bodies, although some of them were the labours of such who had spent the greatest part of their lives in employments relating to the artillery. Such were the tables of *Ufano*, of *Galeus*, of *Ulrick*, &c. taken notice of by *Blondel** : to which might be added many more not mentioned by that author. Indeed there have been very few ancient writers on this subject (and they are a numerous sect) who have not indulged themselves in some speculations on the

* Note, the opinion discussed by *Blondel*, in his *Art de jetter les Bombes*, cap. v. is not originally of *Rivaltius*, whom *Blondel* quotes for it, but of the last-mentioned *Santbech*, from whom *Rivaltius* stole it. Vid. *Santbech*, sect. 6.

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the difference betwixt natural, violent, and mixt motions; although in the application of these mistaken notions scarce any two of them agreed.

But what is most strange, is, that, during these contests, so few of those who were entrusted with the charge of artillery should think it worth while to examine their respective theories by proper experiments. However, thus it has happened; for I do not remember to have met with more than four authors who have actually tried the ranges of shot and shells at different elevations. The first of these is *Collado*, who has given us the ranges of a falconet carrying a three pound shot to each point of the gunner's quadrant: but, from his numbers, it is manifest that the piece was not charged with its customary allotment of powder*. The next is our countryman, *Bourne*, in

* The result of his trials was, that the point blank shot extended 268 paces. At an elevation of one point (which is the twelfth part of the quadrant, or 7½°) the range was 594 paces; at an elevation of two points, the range was 794 paces; at three points, 954 paces; at four points, 1010 paces; at five points, 1040 paces; and at six points, 1053 paces. The range at the seventh point fell between those of the third and fourth; at the eighth point it fell between the ranges of the second and third; at the ninth point it fell between the ranges of the first and

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in a treatise printed the next year after *Collado*. His elevations were not regulated by the points of the gunner's quadrant, but by degrees; and he ascertains the proportion between the ranges at different elevations, and the extent of the point blank shot*. But he has not informed us with what piece he made his trials; though, by his proportions, I presume it must have been a small one. It were to be wished that he had set down this circumstance; for we shall hereafter shew, that the relation between the extent of different ranges will vary extremely, according to the velocity and density of the bullet. The other two which have occurred to me are *Eldred* and *Anderson*, both *Englishmen*; the last of these having vitiated his experiments by his too great attachment

and second; at the tenth point it fell between the point blank distance and that of the first point; and at the eleventh point it fell very near the piece. *Vid. Cap.* lxi. And note, that the paces used by this author are not geometrical paces, but common steps, as he informs us *Cap.* xlii.

* If 1 represents the extent of the point blank shot, then, according to this author, the range at 5° will be $2\frac{2}{3}$, at 10° it will be $3\frac{1}{3}$, at 15° it will be $4\frac{1}{3}$, at 20° it will be $4\frac{5}{6}$, and the greatest random will be $5\frac{1}{2}$; which greatest random, he tells us, in a calm day is at 42°; but according to the strength of the wind, and as it favours or opposes the flight of the shot, it may be from 45° to 36°. *Vid.* His art of shooting in great ordnaunce, *Cap.* vii. H.

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tachment to an erroneous theory, I shall have occasion to mention him hereafter. But *Eldred** deserves a better character: his principles were sufficiently simple, and though not rigorously true, they were, within certain limits, near the truth. He has given us the actual ranges of different pieces of artillery, at small elevations, all under ten degrees. His experiments are numerous, and appear to be made with great care and caution; and he has honestly set down some which were not reconcileable to his method; and, upon the whole, seems to have taken more pains, and to have had a juster knowledge of his business, than is to be found in many of his praetical brethren; for they have been generally too much attached to some incorrect theory, or to the common usage which they have always followed, to think of extending their art by proper experiments, or, indeed, to conceive that it was not already complete; it would otherwise have been impossible that positions so little to be reconciled with experience should have held their ground

so

* His book is entitled THE GUNNER'S GLASSE, and the experiments he relates were most of them made at *Dover-Castle*, of which place he was many years master-gunner. The earliest date I find to any of his experiments is 1611, but his book was not published till 1646.

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so long as they have done; a remarkable instance of which is the doctrine which has taken place in this subject, since the time of *Galileo*.

Galileo printed his dialogues on motion in the year 1638, as we have already observed; and in these he has pointed out the general laws observed by nature in the production and composition of motion, and was the first who described the action and effects of gravity on falling bodies; and on these principles he determined that the flight of a cannon-shot, or of any other projectile, would be in the curve of a parabola, unless so far as it were diverted from that track by the resistance of the air: and what inequalities would thence arise, he has proposed the means of examining; for he has described a method of discovering what sensible effects that resistance would produce in the motion of a bullet at some given distance from the piece.

When *Galileo* had thus shewn, that, independent of the resistance of the air, all projectiles would in their flight describe the curve of a parabola, it might have been expected, that those who came after him would have tried how far the real

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real motions of projectiles deviated from a parabolic track, in order thence to have decided whether the resistance of the air was or was not necessary to be attended to in the determinations of gunnery. But, instead of this cautious procedure, the subsequent writers on gunnery have boldly asserted (without an experimental examination) that no considerable variation could arise from the resistance of the air, in the flight of shells or cannon-shot; supporting themselves in this persuasion chiefly by the consideration of the extreme rarity of the air, compared with the dense and ponderous composition of those projected bodies. And hence (this maxim of the inconsiderable effects of the air's resistance to the motion of shells and bullets, being continually repeated and copied by succeeding authors,) it is now become an axiom almost generally acquiesced in, that the flight of these bodies is nearly in the curve of a parabola.

For in the year 1674, our countryman, *Anderson*, published his treatise of *the genuine use and effects of the gun*, in which he proceeds on the principles of *Galileo*, and strenuously asserts the flight of all bullets to be in the curve of a parabola;

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bola; undertaking to answer all objections that could be urged to the contrary. And in the year 1683, Monsieur *Blondel* published at *Paris L'Art de jetter les Bombes*, where the doctrine of *Galileo* is likewise applied to the motion of shells and bullets of all kinds; and the variations of this doctrine, which can arise from the resistance of the air, are particularly mentioned; and, after a long discussion, the author concludes, that they will be so very minute, as scarcely to affect the accuracy of his conclusions*. Also the same subject is treated of in our Philosophical Transactions†, by Dr. *Halley*, who, swayed by the consideration of the very great disproportion between the density of bullets and of the air, thinks it reasonable to believe that the opposition of the air to large metal shot is scarcely discernible; although in small and light shot he acknowledges that it ought and must be accounted for.

In consequence, then, of these opinions about the inconsiderable effects of the air's resistance on heavy shot, and the demonstrations of *Galileo*, that

* Vid. page 345 of the first quarto edition, at the bottom; also page 355, and following.

† Vid. No. 216. p. 68.

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that all projectiles moved in the curve of a parabola, if they were not disturbed by that resistance; it is now an opinion generally advanced by the writers on the theory of Gunnery, that the flight of shot and shells is nearly in the curve of a parabola; for the truth of which, we may appeal to the professed authors on this subject, who have wrote within the last forty years.

But though this hypothesis went smoothly on with those who contented themselves with speculation only, yet *Anderson*, who made a great number of trials, found it impossible to support it without some new modification. For though it does not appear that he ever examined the comparative ranges of either cannon or musquet-shot, when fired with their usual velocities; yet his experiments on the ranges of shells thrown with small velocities (in respect of those last mentioned) convinced him that their whole track was not parabolical, as appears by his treatise, intituled, *To hit a mark*, published in the year 1690. But instead of making the proper inferences from hence, and discovering the resistance of the air to be of considerable efficacy, he, from his great attachment to his first opinions, framed a new

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hypothesis, which was, that the shell or bullet, at its first discharge, flew a certain distance in a right line, from the end of which line only it began to bend into a parabola. And this right line, which he calls the line of the impulse of the fire, he supposes to be the same in all elevations. By this hypothesis (though an indefensible one) it was always in his power, by assigning a proper magnitude to this line of impulse, to reconcile any two shot made at different angles, however opposite they might prove to the common principles. But even this new-modelled theory was not, I believe, confirmed by his following experiments; for he has no where ventured to give us experiments of three ranges made at three different elevations with the same quantity of powder: as finding, I presume, that though by this scheme he could reconcile two jarring ranges, the irregularities of three were insurmountable. And if such inequalities were produced by the resistance of the air in the motion of a shell impelled from a mortar by an inconsiderable quantity of powder, what may not the action of the air be supposed to effect in the motions of bullets, which, being impelled by a full charge of powder through a much longer cylinder, move perhaps three or four

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four times as fast, and consequently undergo near fifty times the resistance, as will be more particularly evinced hereafter.

That the resistance of the air, which acts with such prodigious power on all swift bodies, should be entirely unattended to by the practitioners in Gunnery, is not the only remarkable circumstance which occurs in this enquiry; for after the publication of Sir *Isaac Newton's Philosophiæ Naturalis Principia Mathematica*, it might have been expected that all mathematicians should have been convinced of its energy; since in that immortal work the law and quantity of this resistance to slow motions is determined, and confirmed by many experiments. Indeed the same law, when extended to swift motions, will be defective, and will exhibit the resistance greatly short of what it really comes out by experiment, (of which Sir *Isaac Newton* himself has given us warning*;) yet, even upon his principles, it would appear that the action of the air on bullets is by far too considerable to be neglected. But, notwithstanding this obvious proof of the necessity of

* Vid. Phil. Nat. Prin. Math. p. 351. l. 17.

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of considering the action of the air on military projectiles, I can recollect but one instance where any computations founded on Sir *Isaac Newton's* doctrine, have been applied to these motions*.

To sum up now at once all we here intend to observe on this head, it appears that the modern writers on the art of Gunnery have been very much deceived, in supposing the resistance of the air to be inconsiderable, and thence asserting, that

* Vid. Comm. Acad. Petrop. Tom. 2. p. 338. 339.

† Besides the observations of Sir I. Newton, above noticed, on the resistance of the air to projectiles, and the path they describe through it, other philosophers likewise, and after him, have made some remarks on the same subject. Huygens has shown, that if the resistance were proportional to the velocity of the moving body, the path described would be a kind of logarithmic curve; but that law of the velocity has no place in nature; the true law being as the square of the velocity in slow motions, and in a much higher degree in swift ones. The problem was proposed in 1718, by Dr. Keil, to Mr. John Bernoulli, who gave a kind of solution to it: another was also given in Herman's Phoronomia; and a third by Dr. Brook Taylor: but these solutions were not adapted to practical uses, and their authors did not suspect the resistance of the air was any thing like so great as it really is. But M. Dan. Bernoulli has shown, in the Petersburgh Commentaries, vol. 2, that the resistance of the air has a very great effect on swift motions, such as those of cannon balls; and particularly concludes, from experiment, that a ball which ascends only 7819 feet in the air, would have ascended 58750 feet in vacuo. H.

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that the track of shot and shells of all kinds is nearly in the curve of a parabola; that by this means it has happened, that all their determinations about the flight of shot discharged with considerable degrees of celerity are extremely erroneous, and consequently that the present theory of Gunnery, in this its most important branch, is useless and fallacious.

Now, to obviate in some degree these imperfections in this art, we have undertaken, in the second chapter of the ensuing treatise, not only to confirm what we have here asserted, relating to the falsity of the parabolic motion of these projectiles, but likewise to ascertain the actual degree of resistance which every shot undergoes according to the velocity with which it moves; whence, as the velocity with which the bullet issues from the piece is easily known by the principles delivered in the first chapter, the delineation of the track passed through by the bullet, hereby becomes a geometrical problem, which, indeed, in its utmost extent, is of a very complicate and operose kind; but in the instances which are most frequent in practice, it admits of some very easy approximations, which enable us

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readily to compare the actual ranges of bullets with the result of this theory.

And though such as examine the following treatise with attention, will not, I believe, entertain many doubts of the certainty of the determinations therein contained, yet it might have been expected, perhaps, that the accuracy of those principles should have been still more irrefragably established by experiments on the real ranges of pieces, compared with computations founded on this theory: and, indeed, I did once intend to have added a chapter with this view; but two reasons have diverted me from this design. The first was, the difficulty I found in ascertaining the extended ranges; a difficulty which none but those who shall attempt experiments of the same kind can be judges of. The second reason was an irregularity which intervened in these ranges, and which rendered all my endeavours fruitless; for the same piece, at the same elevation, would convey the bullet to very distant places, so that no two trials agreed with each other; as I have more particularly recited in the 7th proposition of the 2d chapter.

But,

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But, notwithstanding these difficulties, which have hindered me from inserting in the following treatise such experiments on the ranges of shot as might corroborate the theory of resistance there delivered, I have yet resolved to pursue this subject; and I flatter myself that I have invented a method of preventing the last-mentioned inequality from taking place, which unless it can be done, it is sufficiently obvious how fruitless all experiments of this kind must prove. The result of my future trials on this head I intend for a second part to this treatise; in which, besides these experiments on the track described by the flight of bullets, and the necessary geometrical determinations with which they must be compared, I propose to insert many other experiments, which, though of a miscellaneous nature, are yet all of them connected in some degree with the theory or practice of Gunnery. I shall also annex to this second part many maxims and practical precepts which will arise from the preceding principles, and will, I hope, be of some consequence in the future management of artillery. A considerable part of this second work I have already by me, as likewise an apparatus purposely intended for completing it. But those experiments, which are

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yet wanting, will require great leisure and a proper season to execute.

As the following sheets, besides the determination of the quantity of the air's resistance, do likewise contain the theory of the force and action of powder, it may perhaps be expected that I should give some account of what preceding authors have advanced on this subject. But all I have ever met with on this head hath been so vague and indistinct, that it is often difficult to determine the true meaning of the writer. The most intelligible hypothesis on this head, and what seems, indeed, to have been the original of all the others, is that of Monsieur *de la Hire*.

In the history of the *French* Academy of sciences for the year 1702, Monsieur *de la Hire* has supposed that the force of powder may be owing to the increased elasticity of the air contained in and between the grains, in consequence of the heat and fire produced at the time of the explosion. Now, if this air, to whose augmented spring the violence of gunpowder is imputed, be in its natural state at the time when the powder is fired, (and surely what is in the intervals of the

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grains must be allowed to be so) the greatest addition its elasticity could acquire from the flame of the explosion would not amount to five times its usual quantity, as we shall more particularly evince hereafter*; that is, it would not suffice for the two hundredth part of the effort which we have found to be exerted by fired powder.

However, this hypothesis hath given rise to many dissertations and treatises in a neighbouring nation†, and one author in particular conceives he has made a very reasonable postulate, in supposing the elasticity of the air, when heated by the explosion of the powder, to be only a hundred

* Vid. Prop. V. Cap. 1. *of the following treatise.*

† M. John Bernoulli proved that the air in gunpowder is not in its natural, but in a very compressed state; and particularly that such air is at least 100 times denser than natural air: far short of the truth. And M. Papin, in the Philos. Trans. has shown, by experiment, that saltpetre contains a very elastic matter, in which the strength of the powder consists; and that in 6 grains of powder there is at least one grain of pure air, in a very compressed state; which is also far short of the truth. On the other hand, M. Dan. Bernoulli, in his Hydrodynamy, treats pretty fully on the force of gunpowder, in the 10th section; and in particular asserts, that the elastic force of the air contained in the powder, is more than ten thousand times greater than that of natural air; which is about six or seven times more than the truth. H.

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hundred times greater than when it is heated to the degree of boiling water. But, as I think I have shewn the impossibility of accounting for the actual force of power on these principles, I will not detain the reader any longer with a particular recital of the speculations of these different writers; especially as I flatter myself that I have established that theory of the force of powder which is contained in the following sheets, by such decisive experiments, as will render a formal confutation of any other opinion unnecessary.

NEW

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NEW PRINCIPLES OF GUNNERY.

CHAP. I.

Of the Force of Gunpowder.

PROPOSITION I.

Gunpowder, fired either in a Vacuum *or in Air, produces by its Explosion a permanent elastic Fluid.*

If a red-hot iron be included in a receiver, and the receiver be exhausted, and gunpowder be then let fall on the iron, the powder will take fire, and the mercurial gage will suddenly descend upon the explosion; and though it immediately ascends again, yet it will never rise to the height it first stood at, but will continue depressed by a space proportioned to the quantity of gunpowder which was let fall on the iron. This is a well-known experiment, and is circumstantially described by Mr. *Hauksbee*, in the *Philosophical Transactions*, No. 295; in which place he tells us, that he by this means (firing small quantities at a time) reduced the gage from $29\frac{1}{4}$ inches to $12\frac{1}{4}$. Now this experiment, which has been often repeated, proves the proposition with respect to the production of a permanent elastic fluid in a *vacuum*; for the descent of the gage could only be effected by the pressure of some new generated fluid in the receiver,

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ceiver, balancing in part the pressure of the external air. That this fluid, or some part of it at least, was permanent, appears from what Mr. *Hauksbee* relates in the same place; that though the quicksilver ascended after the operation, yet it next day had ascended no higher than to $22\frac{1}{2}$, at which place it seemed to continue fixed. And that this fluid is elastic, is proved from the descent of the mercurial gage; since the quantity of matter contained in this fluid could not by its gravity alone have sunk the quicksilver by the least sensible quantity; also from its extending itself through any space, however great; the experiment succeeding in either a large or small receiver; only the larger the receiver, the less will be the descent of the mercurial gage to the same quantity of powder, the pressure of the generated fluid diminishing as its density diminishes.

The same production likewise takes place when gunpowder is fired in the air*; for if a small quantity of powder be placed in the upper part of a glass tube, and the lower part of the tube be immerged in water, and the water be made to rise so near the top, that only a small portion of air is left in that part where the gunpowder is placed; if in this situation the communication of the upper part of the tube with the external air be closed, and the gunpowder be fired (which may easily be done by a burning-glass) the water will in this experiment descend on the explosion, as the quicksilver did in the last, and will always continue depressed below the place at which it stood before the explosion; and the quantity of this depression will be greater, if the quantity of powder be increased, or the diameter of the tube be diminished. From whence it is proved, that, as well in air, as in a *vacuum*, the explosion of fired powder produces a permanent elastic fluid.

SCHOLIUM.

* Vid. *Hauksbee* Phys. Mechan. Exper. page 81.

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SCHOLIUM.

It has been known, ever since the time of Mr. *Boyle*, that many substances in fermentation and other chymical operations, produce elastic fluids analogous in some of their effects to the common air. It is likewise known that other mixtures will in many cases absorb a part of the air contiguous to them; in particular, it is observed that all burning bodies, and all sulphureous fumes, destroy great quantities of air, either by absorbing it into their own substance, or at least by depriving it of its elasticity. This creation and consumption of air in chymical processes, has been lately most diligently and successfully examined by the reverend Mr. *Hales*, in his *Vegetable Statics*. And on these principles it follows, that, in the last experiment, the sulphureous fumes arising from the burning of the charcoal and brimstone, contained in the powder, must soon absorb some of the air in which the powder is fired; for which reason it is necessary that the bulk of the air, which the powder is placed in before it is fired, should bear as small a proportion as possible to the quantity of powder; so that the success of the experiment may not be disturbed by the absorbed air approaching to an equality with the generated fluid.

There is, besides, another reason, that, when powder is fired in the manner of the last experiment, the bulk of the air in which it is placed should be as little as possible; which is, that the fire, at the instant of the explosion, will greatly augment the elasticity of that air, and the pressure arising from this increased elasticity, being added to the force of the generated fluid, will endanger the bursting of the tube.

PROP.

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PROP. II.

To explain more particularly the Circumstances attending the Explosion of Gunpowder, either in a Vacuum *or in Air, when fired in the Manner described in the Experiments of the last Proposition.*

When any considerable quantity of Gunpowder is fired in an exhausted receiver, by being let fall on a red-hot iron, the mercurial gage instantly descends upon the explosion, and as suddenly ascends again; and after a few vibrations, none of which, except the first, are of any great extent, it seemingly fixes at a place lower than where it stood before the explosion; and this stationary point is what we have always attended to in our experiments. But even when the gage has acquired this point of apparent repose, it still continues rising for a considerable time, although by such imperceptible degrees, that it can only be discovered by comparing together its place at distant intervals; however, it will not always continue to ascend, but will rise slower and slower, till at last it will be absolutely fixed at a point lower than where the mercury stood before the explosion.

The same circumstances nearly happen when powder is fired in the upper part of an unexhausted tube, whose lower part is immerged in water.

Now these appearances all arise from the different modifications which the fluid, produced from the explosion, undergoes. The first sudden descent of the mercury is effected by the action of that fluid, while in the form of flame. When the flame is extinguished, and consequently the heat of the fluid is diminished, its elasticity is likewise diminished; and this being effected in a very short time, occasions the sudden rise of the mercury after the

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the first descent. When the fluid is reduced to the temperature of the containing receiver, its elasticity is then more fixed and invariable; and this must usually happen by the time the mercurial gage first appears to be stationary. The subsequent slow ascent of the mercury is partly owing to the decrease of the heat of the receiver, occasioned by the cooling of the hot iron contained in it, but much more to the action of the sulphureous fumes of the brimstone and charcoal, which absorb a part of the generated fluid, and thereby diminish its pressure on the gage.

SCHOLIUM.

In the following propositions we shall irrefragably demonstrate, that the force of fired gunpowder is nothing more than the pressure of the fluid, which is generated in the preceding experiments, and that this fluid in its action observes the same laws with other elastic fluids, particularly the air; so that whatever power is produced by the firing of a given quantity of gunpowder, the same would be exerted by substituting in its stead a quantity of air equal to the fluid generated in the explosion, provided that air be included in the same space, and be heated to the same degree, as the other fluid is at the instant of its firing. Mr. *Hales* has concluded, that the weight of the factitious elastic fluids, produced from chymical processes, is the same with that of common air; he having tried that produced from tartar with great exactness. He has found, too, that they expand with heat, and contract with cold, and that with the same pressure they are condensed in the same degree with common air; and that when they are cleared of their sulphureous fumes, which is done by making them pass through water, they will then continue for many months, nay years, without losing any considerable part of their elasticity.

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ticity. And, from these and other circumstances, he doubts not to assert, that these fluids are true permanent air. Now if this be supposed of all, or any of the elastic fluids produced by distillation, burning, &c. it must be preferably allowed to be true of that fluid, which is generated in the explosion of gunpowder; since it is from saltpetre alone that this fluid seems to be derived, (for neither the brimstone nor the charcoal yield it, when fired by themselves) and saltpetre is known to be a substance imbibed from the air by earth; for the same parcel of earth, by being properly exposed to the air, will furnish saltpetre over and over again for ever. However, though it be highly reasonable to suppose that the elastic fluid, arising from the firing of powder, is genuine and permanent air, yet the truth or falshood of this supposition no ways affects the certainty of our conclusions. It is sufficient for our purpose, that it is an elastic fluid; whether it be air or another composition, our reasoning will be still the same; since it is by experiments on this fluid itself, and not by obscure speculations on its nature and qualities, that our future deductions relating to its force and action are confirmed.

PROP. III.

The Elasticity or Pressure of the Fluid produced by the firing of Gunpowder, is cæteris paribus *directly as its Density.*

THIS follows from hence, that, if in the same receiver a double quantity of powder be let fall, the mercury will subside twice as much as in the firing of a single quantity. For the vapour produced from the double quantity being contained in the same receiver will be of double the density of that produced from the single quantity; whence the elasticity or pressure estimated by the descent of the mercury being likewise double, the pressure is

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is directly as its density. Also the descents of the mercury, when equal quantities of powder are fired in different receivers, are reciprocally as the capacities of those receivers, and consequently as the density of the produced fluid in each.

But as in the usual method of trying this experiment, the quantities of powder are so very small, that it is difficult to ascertain these proportions to a requisite degree of exactness, I took a large receiver, containing about 520 inches, and letting fall at once on the red-hot iron, 1 dram or the $\frac{1}{176}$ of an ounce avoirdupois of powder (the receiver being first nearly exhausted) the mercury after the explosion was subsided 2 inches exactly, and all the powder had taken fire. Then heating the iron a second time, and exhausting it as before, 2 drams were let down at once, which sunk the mercury $3\frac{3}{4}$, and a small part of the powder had fallen beside the iron, which (the bottom of the receiver being wet) did not fire, and the quantity that thus escaped did appear to be nearly sufficent, had it fallen on the iron, to have sunk the mercury $\frac{1}{4}$ part of an inch more; in which case the two descents, *viz.* 2 inches and 4 inches, would have been accurately in the proportion of the respective quantities of powder; from which proportion, as it was, they very little varied.

Hence then it appears, that the elasticity of the vapour produced by gunpowder in its explosion, is directly as its density.

PROP. IV.

To determine the Elasticity and Quantity of this elastic Fluid, produced from the Explosion of a given Quantity of Gunpowder.

As different kinds of gunpowder produce different quantities of this fluid in proportion to their different degrees of goodness, before any definite

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determination of this kind can take place, it is necessary to ascertain the particular species of powder that is proposed to be used; and therefore I shall, in every examination and position relating to this subject, suppose the powder in question to be of the same sort with what is made for the use of the government; that being by contract to consist of a known and invariable proportion of materials, and is therefore much properer for a standard than what is compounded according to the arbitrary fancy of the artist.

This being settled, we must further premise these two principles, which we have already mentioned in the scholium to prop. II. the first, that the elasticity of this fluid increases by heat, and diminishes by cold, in the same manner as that of the air; the second, that the density of this fluid, and consequently its weight, is the same with the weight of an equal bulk of air, having the same elasticity, and the same temperature.

Now, from the experiment recited in the last proposition, it appears that $\frac{1}{176}$ of an ounce avoirdupois, or about 27 grains troy, of powder, sunk the gage on its explosion 2 inches; and the mercury in the barometer standing at near 30 inches, $\frac{15}{176}$ of an ounce avoirdupois, or 410 grains troy, would have filled the receiver with a fluid, whose elasticity would have been equal to the whole pressure of the atmosphere, or the same with the elasticity of the air we breathe; and the content of the receiver being about 520 cubic inches, it follows, that $\frac{15}{176}$ of an ounce of powder will produce 520 cubic inches of a fluid, possessing the same degree of elasticity with common air; whence an ounce of powder will produce near 575* cubic inches of such a fluid.

But, in order to ascertain the density of this fluid, we must consider what part of its elasticity, at

* This should be 555. H.

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at the time of this determination, was owing to the heat it received from the included hot iron and the warm receiver. Now the general heat of the receiver being manifestly less than that of boiling water, which is known to increase the elasticity of the air somewhat more than $\frac{1}{4}$ of its augmented quantity, I collect from hence, and other circumstances, that the augmentation of elasticity arising from this cause was about the $\frac{1}{5}$ of the whole; that is, if the fluid arising from the explosion had been reduced to the temperature of the external air, the descent of the mercurial gage, instead of 2 inches, would have been only $1\frac{3}{5}$ inch; whence 575*, reduced in the proportion of 5 to 4, becomes 460†; and this last number represents the cubic inches of an elastic fluid, equal in density and elasticity with common air, which are produced from the explosion of 1 ounce avoirdupois of gunpowder, the weight of which quantity of fluid, according to the usual estimation of the weight of air, is 131 grains; whence the weight of this fluid is $\frac{1}{4}\frac{3}{3}\frac{1}{7}$ or $\frac{3}{1\,0}$ nearly of the weight of the generating powder.

If the ratio of the bulk of the gunpowder to the bulk of this fluid be wanted, this will be determined by knowing that 1 ounce, 1 dram, or 17 drams avoirdupois of powder, fill 2 cubic inches, if the powder be well shook together; wherefore augmenting the number last found in the proportion of 16 to 17, the resulting term $488\frac{3}{4}$‡ is the number of cubic inches of an elastic fluid, equal in density with the air produced from 2 cubic inches of powder; whence the ratio of the respective bulks of the powder, and the fluid produced from it, is in round numbers, 1 to 244§.

And

* This should be 555.
† More correctly 444.
‡ More accurately 472.
§ 236.

H.

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And farther, to confirm this determination, I fired the quantity of a dram of powder four times successively, in an exhausted receiver, by a burning-glass; the capacity of this receiver was 470 cubic inches. These experiments were more troublesome than those in which it was fired by a hot iron, because it was sometimes long before it would fire; in which interval the air would often insinuate itself, and thereby disturb the measures of the descent; and, besides, near $\frac{1}{4}$ part of the powder was usually dissipated, unfired, by the blast: however, by collecting the grains that were thus scattered, and weighing them, and increasing the descent by a proportional quantity, the subsiding of the mercury, corresponding to one dram of powder, was the first time 2,1 + inches, the second time 1,8 — inches, the third time 2, 1—, and the fourth time 1,85 inches, or at a medium 1,96 inches; and this, diminished in the ratio of 520 to 470, becomes 1,77 for the descent to a like quantity in the first receiver. Now the deduction to be made on account of the heat of the receiver was but little in these experiments; for, by including a small thermometer, I found that the fluid within the receiver was not hotter after the blast than that of the summer air; whence, if the descent 1,77 be reduced in the ratio of 13 to 12, which is nearly that of the elasticity of hot summer air to temperate air, it becomes 1,63 nearly, which differs little from $1\frac{3}{5}$, or 1,6; which is what we found it to be in the preceding experiment: whence the proportion between the respective bulks of the powder, and the fluid produced from it, may be still assumed to be that of 1 to 244 [or 236.]

And this ratio agrees very well with the experiment recited by Mr. *Hauksbee*, in his *Physico-Mechanic Experiments*, p. 81; for he there found that one grain of powder produced, when fired in the air, a cubic inch of elastic fluid, which, supposing

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posing the density of powder to be what we have here assigned, gives the ratio of their respective bulks to be that of 1 to 232; a difference, from what we have assigned above, that may easily arise from the difference of the powder only. Whence we may conclude, that the presence of a greater or less quantity of air does not affect the production of this fluid; since, by comparing Mr. *Hauksbee's* experiment with our own, it appears that the same quantity of this fluid is generated in a *vacuum* as in the air.

If this fluid, instead of expanding when the powder was fired, had been confined in the same space which the powder filled before the explosion, then (its elasticity having been shewn to be as its density) it would have had, in that confined state, a degree of elasticity 244 [or 236] times greater than that of common air; and this independent of the great augmentation this elasticity would receive from the action of the fire in that instant*.

Hence, then, we are certain, that any quantity of powder fired in any confined space which it adequately fills, exerts, at the instant of its explosion, against the sides of the vessel containing it, and the bodies it impels before it, a force at least 244 [or

* But it has been found, by other experiments, that 2 cubic inches of powder in grains, occupied only about half that space, or one inch, while in a solid state, or cake, before broken into grains; therefore the space occupied by the fluid, when expanded to the rarity of the atmosphere, would be 236 × 2, or 472 times the bulk of the *solid* powder from which it is produced.—But a solid mass, supposed to be composed of nitre, sulphur, and charcoal only, in the same proportions as employed in making gunpowder, would be very nearly of the same specific gravity as solid gunpowder itself; which contains also air in the proportion of 3 tenths of the whole weight of the powder: it follows therefore, that the condensation of the air in the powder is 472 × $\frac{10}{3}$, or 1573 times, that is, 1573 times denser than common air, and therefore denser than water, in the ratio of about 18 or 19 to 10. H.

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[or 236] times greater than the elasticity of common air, or, which is the same thing, than the pressure of the atmosphere; and this without considering the great addition which this force will receive from the violent degree of heat with which it is endued at that time; the quantity of which augmentation is the next head of our enquiry.

PROP. V.

To determine how much the Elasticity of the Air is augmented, when heated to the extremest Heat of red-hot Iron.

To fix this point, I took a piece of a musket-barrel, about six inches in length, and ordered one end to be closed up entirely; but the other end was drawn out conically, and finished in an aperture of about $\frac{1}{8}$ of an inch in diameter. This tube, thus fitted, was heated to the extremity of a red heat in a smith's forge, and was then immerged with its aperture downwards in a bucket of water, and kept there till it was cool; after which it was taken out carefully, and the water which had entered it in cooling was exactly weighed. The weight of the water thus taken in at three different trials was 610 grains, 595 grains, and 600 grains respectively. The content of the whole cavity of the tube was 796 grains of water; whence the spaces remaining unfilled in these three experiments, were equal in bulk to 186, 201, 196 grains of water respectively; and these spaces did doubtless contain all the air, which, when the tube was red-hot, did extend through its whole concavity; consequently the elasticity of the air, when heated to the extreme heat of red-hot iron, was to the elasticity of the same air, when reduced to the temperature of the ambient atmosphere, as the whole capacity of the tube to the respective spaces taken up by the cooled air; that is, as 796 to 186, 201, 196;

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196; or, taking the medium of these three trials, as 796 to 194$\frac{1}{7}$*.

The heat given to the tube each time was the beginning of what workmen call a white heat; and to prevent the rushing in of the aqueous vapour at the immersion, which will otherwise drive out great part of the air, and render the experiment fallacious; I had an iron wire filed tapering, so as to fit the aperture of the tube, and with this I always stopt it up, before it was taken from the fire, letting it remain in till the whole was cool, when removing it, the due quantity of water would enter.

PROP. VI.

To determine how much that Elasticity of the Fluid produced by the firing of Gunpowder, which we have above assigned, is augmented by the Heat it has at the Time of its Explosion.

As air and this fluid appear to be equally affected by heat and cold, and consequently have their elasticities equally augmented by the addition of equal degrees of heat to each; if we suppose the heat, with which the flame of fired powder is endued, to be the same as that of the extreme heat of red-hot iron, then the elasticity of the generated fluid will be greater at the time of explosion, when it is in the form of flame, than afterwards, when it is reduced to the temperature of the ambient air, in the ratio of 796 to 194$\frac{1}{7}$ nearly; that is, in the ratio of the elasticities of common air, under similar circumstances, ascertained in the last proposition.

Now that the heat of powder, when fired in any considerable quantity, is not less than that of red-hot

* Or the ratio nearly of 4$\frac{1}{11}$ to 1. H.

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hot iron, seems sufficiently evident from the appearance of the flame, and the known properties of some of its materials; for the fire produced by the explosion is certainly as active as any common fire; and it is well known that all fires will communicate a red-hot heat to iron, provided the bulk of the iron be sufficiently small, when compared with the quantity of the fire.

This being supposed, then, that the flame of fired gunpowder is not less hot than red-hot iron, and the elasticity of the air, and consequently of the fluid generated by the explosion, being augmented by the extremity of this heat in the ratio of $194\frac{1}{3}$ to 796, as has been shewn in the last proposion; it follows, that if 244 [or 236] be augmented in this ratio, the resulting number, which is $999\frac{1}{3}$ [or $966\frac{1}{3}$,] will determine how many times the elasticity of the flame of fired powder exceeds the elasticity of common air, supposing it to be confined in the same space which the powder filled before it was fired. For since we have shewn, in the third proposition, that the elastic fluid produced from the firing a quantity of powder, would, if confined in the same space which the powder took up before its explosion, exert an elasticity 244 [236] times greater than the elasticity of common air, supposing the temperature of that fluid and of the air to be the same; it is plain from hence, that, when 244 [236] is increased in the ratio, in which the elasticity of this fluid is greater at the time of the explosion than afterwards, the resulting number will ascertain how many times the elasticity of this inflamed fluid, at the instant of its explosion, and before it has dilated itself, exceeds the elasticity of common air.

Hence, then, the absolute quantity of the pressure exerted by gunpowder, at the moment of its explosion, may be assigned; for, since the fluid, then generated, has an elasticity of $999\frac{1}{3}$ [or 966,]

or

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or in round numbers, 1000* times greater than common air; and since common air, by its elasticity, exerts a pressure on any given surface equal to the weight of the incumbent atmosphere, with which it is *in equilibrio*, the pressure exerted by fired powder, before it has dilated itself, is nearly one thousand times greater than the pressure of the atmosphere; and consequently the quantity of this force on a surface of an inch square, amounts to above 6 ton weight; which force, however, diminishes, as the fluid dilates itself, according to what has been shewn in the third proposition.

SCHOLIUM.

Though we have here supposed that the heat of gunpowder, when fired in any considerable quantity, is the same with iron heated to the extremity of a red heat, or to the beginning of a white heat, (which determination we shall hereafter confirm by many experiments) yet it cannot be doubted, but that the fire produced in the explosion is somewhat varied (like all other fires) by a greater or less quantity of fuel; and it may be presumed, that, according to the quantity of powder fired together, the flame may have all the different degress from that of a languid red heat to the heat sufficient for the vitrification of metals; but as the quantity of powder requisite for the production of this last-mentioned heat, is certainly greater than what is ever fired together for any military purpose, we shall find, by our future experiments, that we shall not be far from our scope, if we suppose the heat of such quantities as come more frequently in use, to be, when fired, nearly the same with the strongest heat of red-hot iron; allowing a gradual augmentation to this heat in larger quantities, and diminishing it when the quantities are very small.

PROP.

* The number corresponding to this, which I have found by many experiments, is near 1600. H.

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PROP. VII.

Given the Dimensions of any Piece of Artillery, the Density of its Ball, and the Quantity of its Charge, to determine the Velocity which the Ball will acquire from the Explosion, supposing the Elasticity of the Powder at the first Instant of its firing to be given.

In the solution of this problem, we shall assume the two following principles:

I. That the action of the powder on the bullet ceases as soon as the bullet is got out of the piece.

II. That all the powder of the charge is fired, and converted into an elastic fluid, before the bullet is sensibly moved from its place.

These postulates we shall demonstrate in an annexed *Scholium*; and they being supposed, the proposition itself is thus determined.

Let AB represent the axis of any piece of artillery, A the breech, and B the muzzle; DC the diameter of its bore, and DEGC a part of its cavity filled with powder. Suppose the ball that is to be impelled to lie with its hinder surface at the line GE, then the pressure exerted at the explosion, on the circle of which GE is the diameter, or, which is the same thing, the pressure exerted in the direction FB, on the surface of the ball, is easily known from the known dimensions of that circle; draw any line FH perpendicular to FB, and AI parallel to FH, and through the point H, to the asymptotes IA and AB, describe the hyperbola KHNQ; then if FH represents the force impelling the ball at the point F, the force impelling the ball in any other place as M will be represented by the line MN, the ordinate to the hyperbola at that point;

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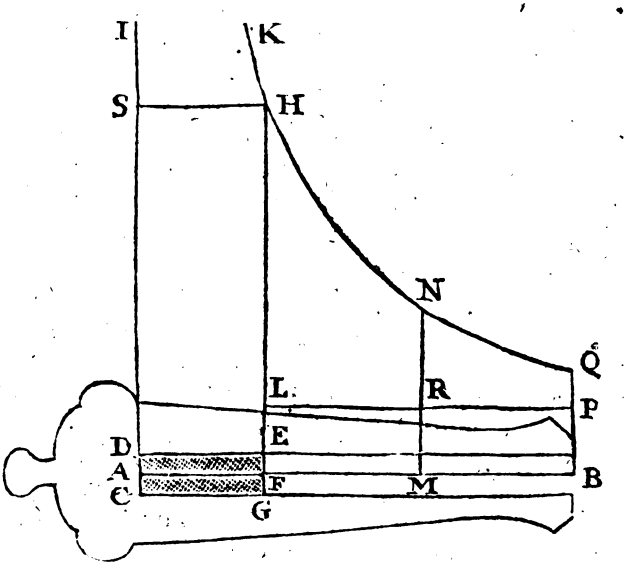
point; for when the fluid impelling the body along, has dilated itself to M, its density will be then to its original density in the space DEGC reciprocally as the spaces through which it is respectively extended; that is, as FA to MA, or as MN to FH; but we have shewn, in the second proposition, that the elasticity or impelling force of this fluid is directly as its density; therefore, if FH represents that force at the point F, MN will represent the like force at the point M.



Since the absolute quantity of the force impelling the ball at the point F is known, and the weight of the ball is likewise known; the proportion between the force the ball is impelled with, and its own gravity, is known. In this proportion take FH to FL, and draw LP parallel to FB; then MN, the ordinate to the hyperbola in any point, will be to its part MR, cut off by the line LP, as the impelling force of the powder in that point M, to the gravity of the ball; and consequently the line LP will determine a line proportional

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tional to the uniform force of gravity in every point; whilst the hyperbola HNQ determines in like manner such ordinates as are proportional to the impelling force of the powder in every point; whence, by the 39th proposition of lib. 1. of Sir *Isaac Newton's Phil. Nat. Prin. Math.* the areas FLPB and FHQB are in the duplicate proportion of the velocities which the ball would acquire, when acted on by its own gravity through the space FB, and when impelled through the same space by the force of the powder. But since the ratio of AF to AB, and the ratio of FH to FL are known, the ratio of the area FLPB to the area FHQB is known; and thence its subduplicate. And since the line FB is given in magnitude, the velocity which a heavy body would acquire, when impelled through this line by its own gravity, is known, being no other than the velocity it would acquire by falling through a space equal to that line; find then another velocity, to which this last-mentioned velocity bears the given ratio of the subduplicate of the area FLPB to the area FHQB, and this velocity, thus found, is the velocity the ball will acquire when impelled through the space FB by the action of the inflamed powder.

Now, to give an example of this, let us suppose AB, the length of the cylinder, to be 45 inches, its diameter DC, or rather the diameter of the ball, to be $\frac{3}{4}$ of an inch; and AF, the extent of the powder, to be $2\frac{1}{4}$ inches; to determine the velocity which will be communicated to a leaden bullet by the explosion, supposing the bullet laid at first with its surface contiguous to the powder.

By the theory we have laid down in the last proposition, it appears, that at the first instant of the explosion, the flame will exert, on the bullet lying close to it, a force 1000 times greater than the pressure of the atmosphere: the medium pressure of the atmosphere is esteemed equal to that of a column

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a column of water 33 feet high; whence lead being to water as 11,345 to 1, this pressure will be equal to that of a column of lead 34,9 inches in height: whence multiplying this by 1000, a column of lead 34900 inches high would produce a pressure equal to what is exerted on the ball by the powder in the first instant of the explosion; and the leaden ball being $\frac{3}{4}$ of an inch in diameter, and consequently equal to a cylinder of lead on the same base, $\frac{1}{2}$ an inch in height, the pressure at first acting on it will be equal to 34900 × 2 or 69800 times its weight; whence FL to FH is as 1 to 69800: and FB to FA is as 45—$2\frac{5}{8}$ (or $42\frac{3}{8}$) to $2\frac{5}{8}$; that is, as 339 to 21; whence the rectangle FLPB is to the rectangle AFHS, as 339 to 21 × 69800; that is, as 1 to 4324—. And from the known application of the logarithms to the mensuration of the hyperbolic spaces, it follows, that the rectangle AFHS is to the area FHQB, as ,43429 &c. is to the tabular logarithm of $\frac{AB}{AF}$; that is, of $\frac{360}{21}$, which is 1,2340579; whence the ratio of the rectangle FLPB to the hyperbolic area FHQB, is compounded of the ratios of 1 to 4324—, and of ,43429 &c. to 1,2340579; which together make up the ratio of 1 to 12263, the subduplicate of which is the ratio of 1 to 110,7; and in this ratio is the velocity which the bullet would acquire by gravity, in falling through a space equal to FB, to the velocity the bullet will acquire from the action of the powder, impelling it through FB; but the space FB being $42\frac{3}{4}$ inches, the velocity a heavy body will acquire in falling through such a space, is known to be what would carry it nearly at the rate of 15,07 feet in 1″ of time; whence the velocity, to which this has the ratio of 1 to 110,7, is a velocity which would carry the ball at the rate of 15,07 × 110,7 feet in 1″ of time; that is, at the rate of 1668 feet in 1″ of time. And this is the velocity which, according to the theory, the bullet in

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in the present circumstances would acquire from the action of the powder, during the time of its dilatation.

And this being once computed for one case, is easily applied to any other; for, if the cavity DE GC left behind the bullet be only in part filled with powder, then the line HF, and consequently the *area* FHQB, will be diminished in the proportion of the whole cavity to the part filled; if the diameter of the bore be varied, the lengths AB and AF remaining the same, then the quantity of powder and the surface of the bullet, which it acts on, will be varied in the duplicate proportion of the diameter; but the weight of the bullet will vary in the triplicate proportion of the diameter; wherefore the line FH, which is directly as the absolute impelling force of the powder, and reciprocally as the gravity of the bullet, will change in the reciprocal proportion of the diameter of the bullet — If AF, the height of the cavity left behind the bullet, be increased or diminished, the rectangle of the hyperbola, and consequently the *area* corresponding to ordinates in any given ratio, will be increased or diminished in the same proportion.— From all which it follows, that the *area* FHQB, which is in the duplicate proportion of the velocity of the impelled body, will be directly as the logarithm $\frac{AB}{AF}$, (where AB represents the length of the barrel, and AF the length of the cavity left behind the bullet) also directly as the part of that cavity filled with powder, and inversely as the diameter of the bore, or rather of the bullet, likewise directly as AF the height of the cavity left behind the bullet. Consequently the velocity being computed above, for a bullet of a determined diameter, placed in a piece of a given length, and impelled by a given quantity of powder, occupying a given cavity behind that bullet; it follows, that, by means of these ratios, the velocity of any other bullet

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bullet may be thence deduced, the necessary circumstances of its position, quantity of powder, *&c.* being given. Where note, that in the instance of this proposition, we have supposed the diameter of the ball to be $\frac{3}{4}$ of an inch: whence the diameter of the bore will be something more, and the quantity of powder contained in the space DEGC will amount to exactly 12dw, a small wad of tow included*.

SCHOLIUM.

In this proposition we have taken for granted,

1*st*, That the action of the powder on the bullet ceases, as soon as the bullet is got out of the piece.

2*dly*, That all the powder of the charge is fired, before the bullet is sensibly moved from its place.

These assumptions we are now to demonstrate.

The first will, I presume, appear manifest, when it is considered, how suddenly the flame will extend itself on every side, by its own elasticity, when

* An algebraic investigation of the rule for the velocity of the ball, may be seen in vol. 2 of Dr. Hutton's Course of Mathematics, viz. in prob. 17, near the end of the volume, p. 350, edit. 4. And the theorem thence deduced, is this: $v = 2713 \times \sqrt{(\frac{nh}{de} \times \text{log. of } \frac{b}{a})}$ for the velocity with which the ball quits the muzzle of the gun; where d denotes the diameter of the bore, e the specific gravity of the ball, or the weight of a cubic foot of its matter, h the length of the cylinder or bore filled with powder, b the whole length of the gun-barrel, a the length to the hinder part of the ball, and n the first force of the fired powder, which Mr. Robins assumes equal to 1000. But when the force of the inflamed powder is considered as carrying both its own weight and that of the ball before it, the more accurate theorem, there stated, is this, $v = 2713 \sqrt{(\frac{nh}{de + 660a} \times \text{log. of } \frac{b}{a})}$ And by comparing the calculations from this theorem with very accurate experiments, there noticed, the medium value of the first force n, comes out about 1500 or 1600, instead of 1000. See pa. 356 of that book. H.

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when it is once got out of the mouth of the piece; for by this means its force will then be dissipated, and the bullet will be no longer sensibly affected by it.

The second principal is, indeed, less obvious, being contrary to the general opinion of almost all writers on this subject. But, however, it is not less certain. It might, perhaps, be sufficient for the proof of this position, to observe the prodigious compression of the flame in the chamber of the piece. Those who will attend to this circumstance, and to the easy passage of the flame through the intervals of the grains, may soon satisfy themselves, that no one grain contained in that chamber can continue for any time uninflamed, when thus surrounded and violently pressed by so active a fire. However, not to rely on mere speculation in a point of so much consequence, I considered, that, if part only of the powder is fired, and that successively, then by laying a greater weight before the charge, (suppose 2 or 3 bullets instead of one) a greater quantity of powder would necessarily be fired, since a heavier weight would be a longer time in passing through the barrel. Whence it should follow, that two or three bullets would be impelled by a much greater force than one only. But the contrary to this appears by experiment; for firing one, two, and three bullets, laid contiguous to each other, with the same charge respectively, I have found (by a method to be mentioned hereafter) that their velocities were not much different from the reciprocal of the subduplicate of their quantities of matter; that is, if a given charge would communicate to one bullet a velocity of 1700 feet in 1″, the same charge would communicate to two bullets a velocity from 1250 to 1300 feet in 1″, and to three bullets, a velocity from 1050 to 1110 feet in 1″. From hence it appears, that, whether the piece be loaded with a greater or less

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less weight of bullet, the action of the powder is nearly the same; since all mathematicians know, that if bodies containing different quantities of matter are successively impelled through the same space by the same power, acting with a determined force at each point of that space, then the velocities given to those different bodies will be reciprocally in the subduplicate ratio of their quantities of matter. The excess of the velocities of the two and three bullets above what they should have been by this rule, (which are that of 1200 and 980 feet in 1″) does doubtless arise from the flame, which escaping by the side of the first bullet, acts on the surface of the second and third.

Now this excess has in many experiments been imperceptible, and the velocities have been reciprocally in the subduplicate ratios of the number of bullets to sufficient exactness; and where this error has been greater, it has never arisen to an eighth part of the whole; but if the common opinion was true, that a small part only of the powder fires at first, and other parts of it successively, as the bullet passes through the barrel, and that a considerable part of it is often blown out of the piece without firing at all; then the velocity, which three bullets received from the explosion, ought to have been much greater than we have ever found it to be; since the time of the passage of three bullets through the barrel being nearly double the time, in which one passes, it should happen, according to this vulgar supposition, that in a double time a much greater quantity of the powder should be fired, and consequently a greater force should have been produced, than what acted on the single bullet only, contrary to all our experiments.

But further, the truth of the second postulate will be more fully evinced, when it shall appear, as it will hereafter, that the rules founded on this

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supposition ascertain the velocities of bullets impelled by powder, to the same exactness, when they are acted on through a barrel of 4 inches in length only, as when they are discharged from one of four feet.

With respect to the grains of powder, which are often blown out unfired, and which are always urged as a proof of the gradual firing of the charge, I believe *Diego Uffano*, a person of great experience in the art of Gunnery, has given the true reason for this accident; which is, that some small part of the charge is often not rammed up with the rest, but is left in the piece before the wad, and is by this means expelled by the blast of air before the fire can reach it*; I must add, that in the charging of cannon and small arms, especially after the first time, this is scarcely to be avoided by any method, I have yet seen practised. Perhaps, too, there may be some few grains in the best powder of so heterogeneous a composition as to be less susceptible of firing, which I think I have myself observed: these, though they are surrounded by the flame, may be driven out unfired. However, be that as it may, the truth of our position cannot in general be questioned.

Having in this proposition shewn how the velocity, which any bullet acquires from the force of powder, may be computed upon the principles of the theory laid down in the preceding propositions of this treatise; we will next shew, that the actual velocities, with which bullets of different magnitudes are impelled from different pieces, with different quantities of powder, are really the same with the velocities assigned by these computations; and consequently, that this theory of the force of powder, here delivered, does unquestionably ascertain the true action and modification of this enormous power.

But

* Dialog. 20.

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But in order to compare the velocities communicated to bullets by the explosion, with the velocities resulting from the theory by computation; it is necessary, that the actual velocities, with which bullets move, should be capable of being discovered, which yet is impossible to be done by any methods hitherto made public. The only means hitherto practised by others for that purpose, have been either by observing the time of the flight of the shot through a given space, or by measuring the range of the shot at a given elevation; and thence computing on the parabolic hypothesis, what velocity would produce this range. The first method labours under this insurmountable difficulty, that the velocities of these bodies are often so swift, and consequently the time observed is so short, that an imperceptible error in that time may occasion an error in the velocity, thus found, of 2, 3, 4, 5, or 600 feet in a second. The other method is so fallacious, by reason of the resistance of the air, (to which inequality the first is also liable) that the velocities thus assigned may not be, perhaps, the tenth part of the actual velocities sought.

To remedy, then, these inconveniencies, I have invented a new method of finding the real velocities of bullets of all kinds; and this to such a degree of exactness, (which may be augmented too at pleasure) that in a bullet moving with a velocity of 1700 feet in 1″, the error in the estimation of it need never amount to its five hundredth part; and this without any extraordinary nicety in the construction of the machine. The description and use of which is the subject of the next proposition.

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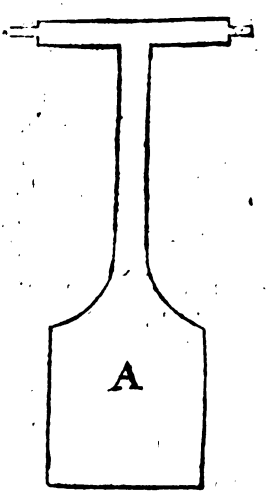
PROP. VIII.

To determine the Velocity, which any Ball moves with at any Distance from the Piece, it is discharged from.

THE simplest method of effecting this, is by means of an instrument like to that exhibited in the engraved figure, where ABCD represents the body of the machine, composed of the three poles B, C, D, spreading at bottom, and joining together at the top A; being the same with what is vulgarly used in the weighing and lifting of very heavy bodies, and is called by workmen the triangles. On two of these poles, towards their tops, are screwed on the sockets RS; and on these sockets the pendulum EFGHIK is hung by means of its cross piece EF, which becomes its axis of suspension, and on which it must be made to vibrate with great freedom. The body of this pendulum is made of iron, having a broad part at bottom, which cannot be seen in this scheme; but its entire shape is represented in the annexed figure A.



The

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The lower part of the pendulum is covered with a thick piece of wood GKIH, which is fastened to the iron by screws. Something lower than the bottom of the pendulum there is a brace OP, joining the two poles to which the pendulum is suspended; and to this brace there is fastened a contrivance MNU, made with two edges of steel, bearing on each other in the line UN, something in the manner of a drawing-pen; the strength with which these edges press on each other being diminished or increased at pleasure, by means of a screw Z going through the upper piece. There is fastened to the bottom of the pendulum a narrow ribbon LN, which passes between these steel edges, and which afterwards, by means of an opening cut in the lower piece of steel, hangs loosely down, as at W.

This instrument thus fitted, if the weight of the pendulum be known, and likewise the respective distances of its centre of gravity, and of its centre of oscillation, from its axis of suspension, it will thence be known, what motion will be communicated to this pendulum by the percussion of a body of a known weight moving with a known degree of celerity, and striking it in a given point; that is, if the pendulum be supposed at rest before the percussion, it will be known, what vibration it ought to make in consequence of such a determined blow; and, on the contrary, if the pendulum, being at rest, is struck by a body of a known weight, and the vibration, which the pendulum makes after the blow, is known, the velocity of the striking body may from thence be determined.

Hence, then, if a bullet of a known weight strikes the pendulum, and the vibration, which the pendulum makes in consequence of the stroke, be ascertained; the velocity, with which the ball moved, is thence to be known.

Now the extent of the vibration, made by the

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pendulum after the blow, may be measured t great accuracy by the ribbon LN; for let the pressure of the edges UN on the ribbon be so regulated by the screw Z, that the motion of the ribbon between them may be free and easy, though with some minute resistance; then settling the pendulum at rest, let the part LN between the pendulum and the edges be drawn straight, but not strained, and fix a pin in that part of the ribbon, which is then contiguous to the edges: let now a ball impinge on the pendulum, then the pendulum swinging back will draw out the ribbon to the just extent of its vibration, which will consequently be determined by the interval on the ribbon between the edges UN, and the place of the pin.

But the computation, by which the velocity of the ball is determined from the vibration of the pendulum after the stroke, requires a more particular explication; and for this purpose we will exhibit, as an example, the pendulum made use of by us in some of our experiments.

The weight of the whole pendulum, wood and all, was 56 lb. 3 oz. its centre of gravity was 52 inches distant from its axis of suspension, and 200 of its small swings were performed in the time of 253 seconds; whence its centre of oscillation (determined from hence) is $62\frac{2}{3}$ inches distant from that axis. The centre of the piece of wood GKIH is distant from the same axis 66 inches.

In the compound ratio of 66 to $62\frac{2}{3}$, and 66 to 52, take the quantity of matter of the pendulum to a 4th quantity, which will be 42 lb. $\frac{1}{2}$ oz. Now geometers will know, that if the blow be struck in the centre of the piece of wood GKIH, the pendulum will resist to the stroke in the same manner, as if this last quantity of matter only (42 lb. $\frac{1}{2}$ oz.) was concentrated in that point*, and the rest

* T' is the case only when that point is the centre of oscillation;

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est of the pendulum was taken away; whence, supposing the weight of the bullet impinging in that point to be the $\frac{1}{114}$ of a pound, or the $\frac{1}{504}$ of this quantity of matter nearly, the velocity of the point of oscillation after the stroke will, by the laws observed in the congress of such bodies as rebound not from each other, be the $\frac{1}{505}$ of the velocity, the bullet moved with before the stroke; whence the velocity of this point of oscillation after the stroke being ascertained, that multiplied by 505 will give the velocity with which the ball impinged.

But the velocity of the point of oscillation after the stroke is easily deduced from the chord of the arch, through which it ascends by the blow; for it is a well-known proposition, that all pendulous bodies ascend to the same height by their vibratory motion, as they would do, if they were projected directly

cillation; a circumstance forgotten to be noticed when this tract was first printed in the year 1742, but which was mentioned by the author, in the Philos. Transactions for the year following, where the rule, is properly corrected, and where we are informed that the first example here given only requires to be corrected, as all the other examples in the 9th prop. following, were computed by the corrected rule. The necessary correction for the above rule, is to increase the velocity in the ratio of the distances of the centres of oscillation and percussion, below the axis of suspension. And that remark, had M. Euler observed it, might have spared him the trouble of many of his animadversions on Mr. Robins's work. As to the rule, in an algebraical form, the easiest that I know of, is that given in my volume of Tracts, printed in 1786, p. 119, viz. $v = 614.58\ g\ c \times \frac{p + b}{b\ i\ r\ n}$ for the velocity of the ball in feet: where b denotes the weight of the ball, p the weight of the pendulum, g the distance to the centre of gravity, i the distance to the point of impact or point struck, c the chord of the arch described by the pendulum, to the radius or distance r, and n the number of small vibrations the pendulum makes in one minute or 60 seconds: and where the values of the quantities c, g, i, r may be taken in any one and the same measure, either all inches, or all feet, &c.; also p and b in any one measure, either pounds or ounces, &c. H.

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directly upwards from their lowest point, with the same velocity they have in that point; wherefore, if the versed sine of the ascending arch be found, (which is easily determined from the chord and radius being given) this versed sine is the perpendicular height, to which a body projected upwards with the velocity of the point of oscillation would arise; and, consequently, what that velocity is, can be easily computed by the common theory of falling bodies.

For instance, the chord of the arch, described by the ascent of the pendulum after the stroke measured on the ribbon, has been sometimes $17\frac{1}{4}$ inches; the distance of the ribbon from the axis of suspension is $71\frac{1}{8}$ inches; whence reducing $17\frac{1}{4}$ in the ratio of $71\frac{1}{8}$ to 66, the resulting number, which is nearly 16 inches, will be the chord of the arch, through which the centre of the board GKIH ascended after the stroke: now the versed sine of an arch, whose chord is 16 inches, and its radius 66, is 1,93939; and the velocity, which would carry a body to this height, or, which is the same thing, the velocity, which a body would acquire by descending through this space, is nearly that of *$3\frac{1}{4}$ feet in 1″.

To determine, then, the velocity, with which the bullet impinged on the centre of the wood, when the chord of the arch described by the ascent of the pendulum, in consequence of the blow, was $17\frac{1}{4}$ inches measured on the ribbon, no more is necessary, than to multiply $3\frac{1}{4}$ by 505, and the resulting number 1641† will be the feet which the bullet would describe in 1″, if it moved with the velocity it had at the moment of its percussion; for the velocity of the point of the pendulum, on which

* More accurately 3,216.

† Or rather $1624 = 3.216 \times 505$. But the true velocity, when computed by the correct rule in the last note, comes out 1669 or near 1700 feet. H.

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which the bullet struck, we have just now determined to be that of $3\frac{1}{4}$ feet in 1″; and we have before shewn, that this is the $\frac{1}{505}$ of the velocity of the bullet. If, then, a bullet weighing $\frac{1}{12}$ of a pound strikes the pendulum in the centre of the wood GKIH, and the ribbon be drawn out $17\frac{1}{4}$ inches by the blow; the velocity of the bullet is that of 1641 feet in 1″. And since the length the ribbon is drawn, is always nearly the chord of the arch described by the ascent, (it being placed so as to differ insensibly from those chords which most frequently occur) and these chords are known to be in the proportion of the velocities of the pendulum acquired from the stroke, it follows that the proportion between the lengths of ribbon drawn out at different times, will be the same with that of the velocities of the impinging bullets; and, consequently, by the proportion of these lengths of ribbon to $17\frac{1}{4}$, the proportion of the velocity with which the bullets impinge to the known velocity of 1641 feet in 1″, will be determined.

Hence, then, is shewn, in general, how the velocities of bullets of all kinds may be found out by means of this instrument; but that those, who may be disposed to try these experiments, may not have unforeseen difficulties to struggle with, I shall here subjoin a few observations, which it will be necessary for them to attend to, both to secure success to their trials, and safety to their persons.

And first, that they may not conceive the piece of wood GKIH to be an unnecessary part of the machine, I must inform them, that if a bullet impelled by a full charge of powder should strike directly on the iron, the bullet would be beaten into shivers by the stroke, and these shivers will rebound back with such violence, as to bury themselves in any wood they chance to light on, as I have found by hazardous experience; and besides the danger, the pendulum will not in this instance ascertain

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ascertain the velocity of the bullet, because the velocity, with which the parts of it rebound, is unknown.

The weight of the pendulum, and the thickness of the wood, must be in some measure proportioned to the size of the bullets which are used.—A pendulum of the weight here described, will do very well for all bullets under three or four ounces, if the thickness of the board be increased to 7 or 8 inches for the heaviest bullets: beech is the toughest and properest wood for this purpose.

It is hazardous standing on the side of the pendulum, unless the board be so thick, that the greatest part of the bullet's force is lost before it comes at the iron; for if it strikes the iron with violence, the shivers of lead, which cannot return back through the wood, will force themselves out between the wood and iron, and will fly to a considerable distance.

As there is no effectual way of fastening the wood to the iron but by screws, the heads of which must come through the board; the bullets will sometimes light on those screws, from whence the shivers will disperse themselves on every side.

When in these experiments so small a quantity of powder is used, as will not give to the bullet a velocity of more than 4 or 500 feet in 1", the bullet will not stick in the wood, but will rebound from it entire, and (if the wood be of a very hard texture) with a very considerable velocity. Indeed I have never examined any of the bullets, which have thus rebounded, but I have found them indented by the bodies, they have struck against in their rebound.

To avoid, then, these dangers, to the braving of which in philosophical researches no honour is annexed; it will be convenient to fix whatsoever barrel is used, on a strong heavy carriage, and to fire it with a little slow match. Let the barrel too

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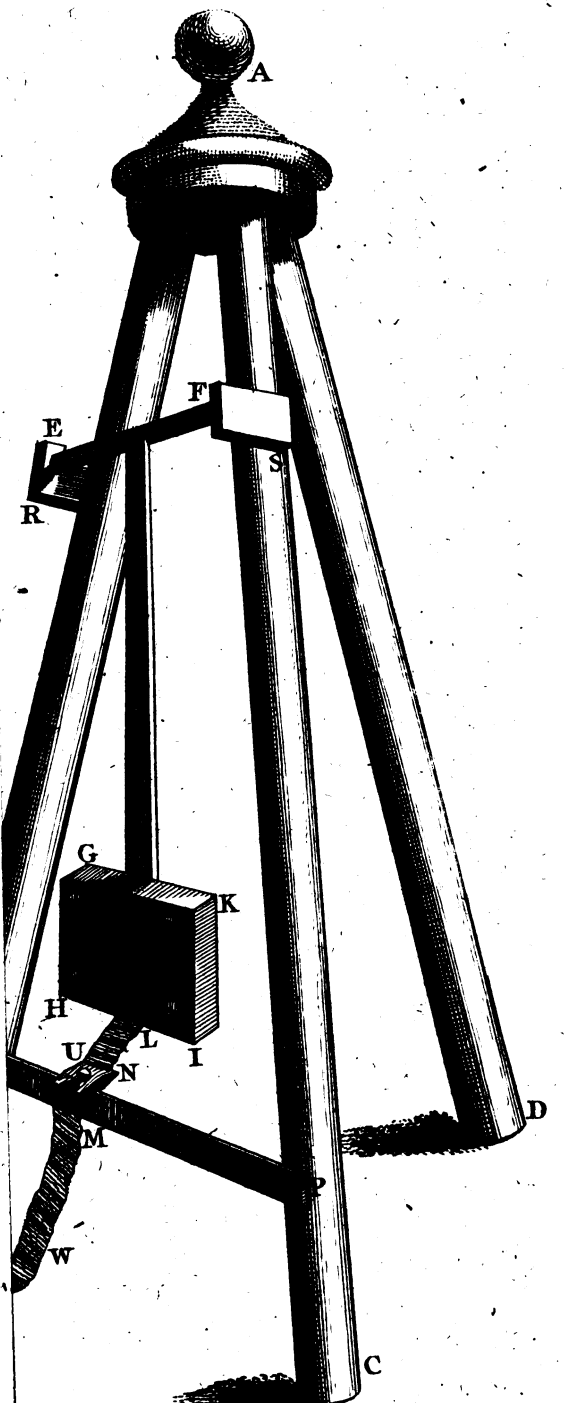

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be very well fortified in all its length; for no barrel (I speak of musket-barrels) forged with the usual dimensions, will bear many of the experiments recited hereafter without bursting, as I have found to my cost. The barrel, I have most relied on, and which I procured to be made on purpose, is nearly as thick at the muzzle as at the breech; that is, it has in each place nearly the diameter of its bore in thickness of metal.

The powder used in these experiments should be exactly weighed, and that no part of it be scattered in the barrel, the piece must be charged with a ladle in the same manner as is practised with cannon, the wad should be of tow of the same weight each time, and no more than is just necessary to confine the powder in its proper place; the length of the cavity left behind the ball should be determined each time with exactness; for the increasing or diminishing that space will vary the velocity of the shot, although the bullet and quantity of powder be not changed. The distance of the mouth of the piece from the pendulum ought to be such, that the impulse of the flame may not act on the pendulum; this will be prevented in a common barrel charged with $\frac{1}{2}$ an ounce of powder, if it be at the distance of 16 or 18 feet; in larger charges the impulse is sensible farther off; I have found it to extend to above 25 feet; however, between 25 and 18 feet is the distance I have usually chosen; other precautions, which are necessary, will better find their place in the account of the experiments I have made, to which I now hasten*.

PROP.

* Many improvements of the machinery, of this ingenious way of discovering the velocity of shot, may be seen in my Tracts, before-mentioned; where the method is extended to the practice with cannon balls of considerable size, and illustrated by very numerous and varied examples. H.

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PROP. IX.

To compare the actual Velocities with which Bullets of different Kinds are discharged from their respective Pieces, with their Velocities computed from the Theory.

How to determine the actual velocities with which bullets are discharged, has been amply shewn in the last proposition; and how to compute the velocity with which they would be discharged according to our theory, has been likewise fully explained in the sixth proposition; we shall here then compare the result of our theory with experience, and thence evince, how accurately that theory agrees with the real motions of bullets, though founded on principles no ways connected with these experiments.

The first experiments, I shall exhibit, were made with a barrel of the same dimensions with the example of the sixth proposition, the ball being $\frac{3}{4}$ of an inch in diameter, the length 45 inches, and the cavity containing the powder $2\frac{3}{4}$ inches, which, as the barrel exceeded the bullet in diameter by about the $\frac{1}{40}$ of an inch, just contained 12dw. of powder.

The bullet thus made use of was $\frac{1}{12}$ of a pound, avoirdupois, in weight, and consequently the same with the example of the seventh proposition; but the board on the pendulum used here was 4lb. lighter than what is assigned in that example; from these circumstances, and the velocity which by the theory the bullet ought to be discharged with, there is known the chord of the arch measured on the ribbon, through which the pendulum should ascend after the stroke, if the theory be true: how near this agrees with our experiments, will appear by the following table:

No.

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No.	Quantity of Powder.	Chord of its ascending arch measured on the ribbon.	The same by the theory.	Error of the theory.
	Dw.			
1	12	18,7	19,0	+,3
2	12	19,6	19,0	—,6
3	6	13,6	13,4	—,2

The next experiments were made with the same barrel, but the board on the pendulum was now of little more weight than that in the example of the seventh proposition.

No.	Length of the cavity containing the powder or line AF, in fig. 1.	Quantity of powder.	Chord of its ascending arch measured on the ribbon.	The same by the theory.	Error of the theory.
	Inches.	Dw.	Inch.	Inch.	Inch.
4	2 5/8	6	11,9	12,1	+,2
5	2 5/8	6	12,2	12,1	—,1
6	1 1/4	6	13,2	13,6	+,4
7	1 1/4	6	13,9	13,6	—,3
8	2 5/8	12	16,7	17,2	+,5
9	2 5/8	12	17,5	17,2	—,3
10	2 5/8	12	16,9	16,8	—,1
11	2 5/8	12	17,0	16,8	—,2
12	2 5/8	6	11,7	11,5	—,2
13	2 5/8	6	11,1	11,5	+,4
14	2 5/8	12	16,7	16,3	—,4

The last five numbers resulting from the theory are corrected from the quantity of bullets lodged in the board, which, as many other experiments of a different kind were tried in the interval, amounted at last to above two pounds; whence the weight of the pendulum being increased, its vibration with the same blow must be proportionably diminished.

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The next experiments were made with a barrel of the same bore with the last, but only 12,375 inches in length: to distinguish them, we shall for the future denominate the first barrel by the letter A, and this short one by C. The board on the pendulum was at first rather lighter than in the example of the seventh proposition.

No.	Bar.	Extent of the cavity containing the powder.	Quantity of powder.	Chord of the ascending arch measured on the ribbon.	The same by the theory.	Error of the theory.
		Inch.	Dw.	Inch.	Inch.	
15	C	2 $\frac{5}{8}$	12	12,7	12,8	+,1
16	C	2 $\frac{5}{8}$	12	12,6	12,8	+,2
17	C	2 $\frac{5}{8}$	12	12,4	12,8	+,4
18	A	2 $\frac{5}{8}$	12	17,0	17,3	+,3
19	A	2 $\frac{5}{8}$	12	17,2	17,2	,0
20	A	2 $\frac{5}{8}$	12	17,1	17,2	+,1
21	A	2 $\frac{5}{8}$	12	17,2	17,2	,0
22	A	2 $\frac{5}{8}$	6	12,4	12,2	−,2

In some of the following experiments a third barrel was used of the same bore with the other two, but 24,312 inches in length: this barrel I denominate B; the board fixed on the pendulum was at first but little heavier than that in the seventh proposition; and when in the course of the experiments it is sensiby increased in the weight, I diminish the numbers arising from the theory by a corresponding part.

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No.	Bar.	Extent of the cavity containing the powder. Inch.	Quantity of powder. Dw.	Chord of the ascending arch measured on the ribbon. Inch.	The same by the theory. Inch.	Error of the theory. Inch.
23	A	2 $\frac{1}{8}$	12	17,1	17,2	+ ,1
24	A	2 $\frac{1}{8}$	9	15,2	15,0	− ,2
25	A	2 $\frac{1}{8}$	9	15,4	15,0	− ,4
26	C	2 $\frac{1}{8}$	12	11,5	12,8	+ 1,3
27	C	2 $\frac{1}{8}$	12	11,5	12,8	+ 1,3
28	C	2 $\frac{1}{8}$	6	8,7	9,	+ ,3
29	C	2 $\frac{1}{8}$	12	12,3	12,5	+ ,2
30	B	2 $\frac{1}{8}$	12	14,4	14,4	0,0
31	B	2 $\frac{1}{8}$	12	14,4	14,4	0,0
32	B	2 $\frac{1}{8}$	6	10,3	10,5	+ ,2
33	A	1 $\frac{3}{4}$	8	14,7	14,5	− ,2
34	A	4	12	15,7	15,3	− ,4

The error in the 26 and 27th experiments being much greater than what has occurred to me in any other trials, I suspect, that some mistake was made in the weight of the powder, or that the barrel (which had indeed lain by in a moist place) was very damp; which circumstance, I know by experience, will considerably diminish the action of the powder.

The following experiments were made with a pendulum much heavier, it weighing in the whole 97lb. its centre of gravity was 55,625 inches distant from its axis of suspension, and 200 of its small swings were performed in the space of $255''\frac{1}{4}$, whence its centre of oscillation is 63,9 inches distant from the axis of suspension. Also sometimes another barrel was used 7,06 inches in length, and ,83 in diameter; its ball was exactly fitted to the bore without any windage, so that it went in with difficulty; the weight of this ball was $33\frac{1}{2}$ dw.— This barrel we shall denominate D.

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No.	Bar.	Length of the cavity containing the powder. Inch.	Quantity of powder. Dw.	Chord of the ascending arch measured on the ribbon. Inch.	The same by the theory. Inch.	Error of the theory. Inch.
35	A	2 5/8	12	9,2	9,2	,0
36	A	2 5/8	12	9,5	9,2	−,3
37	A	5 1/4	24	11,7	11,3	−,4
38	A	7 7/8	36	13,2	12,6	−,6
39	A	2 5/8	12	9,3	9,1	−,2
40	A	1 3/4	8	7,6	8,1	+,5
41	C	2 5/8	12	6,1	6,6	+,5
42	C	2 5/8	12	6,5	6,6	+,1
43	B	2 5/8	12	8,0	8,2	+,2
44	B	2 5/8	12	8,3	8,2	−,1
45	A	2 5/8	12	9,5	9,1	−,4
46	A	2 5/8	12	9,1	9,1	,0
47	A	2 5/8	6	7,2	6,5	−,7
48	A	2 5/8	6	6,7	6,5	−,2
49	C	2 5/8	12	6,8	6,7	−,1
50	C	2 5/8	12	7,5	6,7	−,8
51	C	2 5/8	6	4,7	4,8	+,1
52	C	2 5/8	6	5,0	4,8	−,2
53	D	2 1/4	12	7,0	7,2	+,2
54	D	2 5/8	12	7,1	6,8	−,3
55	D	2 5/8	6	4,7	4,8	+,1
56	D	2 5/8	6	4,8	4,8	,0
57	A	2 1/16	6	6,4	6,5	+,1
58	A	2 1/16	6	6,4	6,5	+,1
59	A	2 1/16	6	6,6	6,5	−,1
60	A	2 1/16	6	6,7	6,5	−,2
61	A	2 1/16	12	9,0	9,1	+,1

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The error in the 50th experiment, the greatest in this set, was doubtless owing to the wind; for the 49, which was made immediately before it in the same manner, and with the same quantity of powder, differs but little from the theory. The excess of the 38th experiment above the theory, was in part occasioned by the impulse of the flame on the pendulum, which in this large quantity of powder was plainly to be discerned.

This theory is farther confirmed too, by experiments made on the action of very small quantities of powder. We have hitherto supposed the powder when fired to be equally hot with iron at the beginning of its white heat, but we have observed, that in very small quantities of powder the heat is probably less than this, and consequently the elasticity in those cases less than what arises from this supposition. Now this decrease of elasticity in small quantities of powder we have found by many trials actually to take place; for instance, in the example of the 7th proposition, the velocity, which should be given to the ball by the action of the powder according to the theory, is in round numbers that of 1670 feet in 1″, and this velocity we have found in the preceding experiments to be the medium velocity, which the ball really receives in those circumstances. If now, the barrel and position of the ball remaining the same, there be placed in the same space DEGC continuing likewise the same 1dw. of powder instead of 12, which is the quantity supposed in that example; it follows from the principles there laid down, that if the elasticity of the smaller charge be the same in proportion to its quantity with that of the larger, then the velocity of the bullet, when impelled by the explosion of the smaller charge, will be to the velocity of a bullet impelled by the greater charge, in the subduplicate ratio of the quantities of the respective charges; that is, in the subduplicate ra-

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tain the true and genuine determination of the force and manner of acting of fired gunpowder.

This theory, as here established, supposes, that in the firing of gunpowder about $\frac{1}{100}$ of its substance is converted by the sudden inflammation into a permanent elastic fluid, whose elasticity, in proportion to its heat and density, is the same with that of common air in the like circumstances; it farther supposes, that all the force exerted by gunpowder, in its most violent operations, is no more than the action of the elasticity of the fluid thus generated; and these principles enable us, as we have seen, to determine the velocities of bullets impelled from fire-arms of all kinds, and are fully sufficient for all purposes, where the force of gunpowder is to be estimated. Whether this fluid be true and genuine air, or another substance, we shall not discuss in this place; as it is an enquiry no ways connected with the design of this treatise.

From this theory many deductions may be made of the greatest consequence to the practical part of Gunnery. From hence the thickness of a piece, which will enable it to confine without bursting any given charge of powder, is easily determined, since the effort of the powder is known. From hence appears the inconclusiveness of what some modern authors have advanced, relating to the advantages of particular forms for the chambers of mortars and cannon; for all their laboured speculations on this head are evidently founded on very erroneous opinions about the action of fired powder. From this theory, too, we are taught the necessity of leaving the same space behind the bullet, when we would by the same quantity of powder communicate the same velocity to the bullet; since on our principles it follows, that the same powder has a greater or less degree of elasticity, according to the different spaces it occupies. The method, which I have always practised for this purpose, has been by marking

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marking the rammer; and this is a maxim, which ought not to be dispensed with, when cannon are fired at an elevation, particularly in those called by the French *Batteries à ricochet.*

From the continued action of the powder, and its manner of expanding described in this theory, and the length and weight of the piece; one of the most essential circumstances in the well-directing of artillery may be easily ascertained. All practitioners are agreed, that no shot can be depended on, unless the piece be placed on a solid platform; for if the platform shakes with the first impulse of the powder, it is impossible but the piece must likewise shake, which will alter its direction, and render its shot uncertain. To prevent this accident, the platform is usually made extremely firm to a considerable depth backwards; so that the piece is not only well supported in the beginning of its motion, but likewise through a great part of its recoil. However, it is sufficiently obvious, that when the bullet is separated from the piece, it can be no longer affected by the trembling of the piece or platform; and by a very easy computation it will be found, that in a piece 10 feet in length, carrying a bullet of 24lb. and charged with 16lb. of powder, the bullet will be out of the piece, before the piece has recoiled $\frac{1}{2}$ an inch; whence, if the platform be sufficiently solid at the beginning of the recoil, the remaining part of it may be much slighter; since its unsteadiness beyond the first $\frac{1}{2}$ inch will have no influence on the direction of the shot. And hence a more compendious method of constructing platforms may be found out.

From this theory it also appears, how greatly those authors have been mistaken, who have attributed the force of gunpowder, or at least a considerable part of it, to the action of the air contained either in the powder or between the intervals of the grains; for they have supposed (though indis-

tinctly enough) that air to exist in its natural elastic state, and to receive all its addition of force from the heat of the explosion. But, from what we have experimented in the fifth proposition, relating to the increase of the elasticity of the air by heat, we may conclude, that the heat of the explosion cannot augment the elasticity of the air to five times its common quantity; consequently, the force arising from this cause only cannot amount to more than the 200th part of the real force exerted on this occasion.

Having thus dispatched the general confirmation of our theory, we shall proceed to the examination of some other particulars relating to this subject; which, though easily enough flowing from the principles already laid down, do yet, from the novelty and singularity of the matter, merit a circumstantial discussion.

PROP. X.

To assign the Changes in the Force of Powder, which arise from the different State of the Atmosphere.

In all the experiments I have hitherto examined, I have never been able to discover, that the variation of the density of the atmosphere did any way alter the action of powder, although I have made several hundred shot in very different seasons: in particular, I have sometimes compared the trials made at noon, in the hottest summer sun, with those made in the freshness of the morning and evening, and I could not perceive any certain difference between them, and it was the same with the trials made in the night and in winter; although in this variety of seasons, the density of the atmosphere must have been very different: indeed, as we have seen that the same quantity of that fluid, in which the force of powder consists, is generated

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in a *vacuum*, and in common air, it is difficult to conceive how this force can be affected by the greater or lesser density of the atmosphere.

But though the density of the atmosphere has no influence on the force of powder, yet its moisture has a great one; for the same quantity of powder, which in a dry season would communicate to a bullet a velocity of 1700 feet in 1″, will not in damp weather communicate to the same bullet, placed in the same manner, a velocity of more, perhaps, than 12 or 1300 feet in 1″, or still less, if the powder be of a bad sort, and has been negligently kept. And this decrease of the force in damp powder appears by my experiments to be very unsteady and variable; so that 2 shot made with equal quantities of such powder, taken out of the same parcel, will differ considerably from each other, perhaps ten times more than if the powder was in good order; and as far as this uncertainty in its effects will permit me, I seem to collect, that a small charge loses a greater part of its force than a larger, provided each are equally damp. Another circumstance attending damp powder, is a remarkable foulness in the piece after firing, much beyond what arises from an equal quantity of dry powder.

Now all these effects are easily to be accounted for, when it is known, that powder will imbibe moisture from the air; for as a certain quantity of water mixed with powder will prevent its firing at all, it follows, that every degree of moisture in powder, though insufficient to produce this effect, will yet abate the violence of its explosion, and will render the fire thereby produced less vehement than it would otherwise be; whence a less quantity of fluid will be in this case generated, and the heat of that fluid and its elasticity is likewise less; consequently the action of damp powder must, on

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this two-fold account, be diminished according to the degree of moisture with which it is impregnated.

And as bad powder usually contains some common salt in it, by reason of the little care taken in the refining of the nitre, and as common salt imbibes moisture in a stronger degree than nitre; it is not difficult to conceive, how bad powder should in a moist season be more impregnated with moisture than good, and should consequently lose more of its force.

The uncertainty in the effects of damp powder arises, I presume, from the different degrees of dryness it acquires in the piece; for as, after the first or second firing, the barrel grows warm, if the powder is contained any time in it, some part of its moisture will be thereby evaporated; and as the heat of the barrel, and the time of the charge continuing in it, are circumstances, which in their nature are very uncertain, it is not to be wondered at, that the evaporation, and consequently the action of the powder, is likewise uncertain. I must remark, on this head, that, in the driest seasons, I have found the coldness of the barrel, and perhaps some little moisture condensed in its cavity, to have sensibly diminished the force of the powder in the first shot.

That small quantities of powder should have their action more diminished than larger quantities with the same degree of moisture, naturally follows from the smaller degree of heat, with which (as we have observed above) the explosion of small charges is attended; since the same proportion of moisture must of necessity clog a weak fire more effectually, than it can do one which is more violent.

The remarkable foulness of the piece, from the firing of damp powder, which we have mentioned above, must likewise arise from the diminution of the activity of the fire in the explosion. For,

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when powder is of a proper temperature to fire readily and violently, the greatest part of its substance ought to be consumed to ashes, which will then be discernible in the form of a greyish substance on all bodies placed near the mouth of the piece; and the foulness of the piece is owing to those parts of the powder, which, either by their contiguity to the cold barrel, or their less inflammable composition, are but imperfectly burnt; now since moist powder produces a less violent flame, in proportion to the moisture it imbibed; it must follow, that a smaller part of the powder will in this case be perfectly consumed, and consequently a greater part will remain to contribute to the foulness of the barrel.

SCHOLIUM.

We have asserted, as the basis of our reasoning in this proposition, that powder will imbibe moisture from the air in a humid state of the atmosphere; but it remains to assign the quantity it can thus imbibe, which we shall here endeavour to do from our own experiments.

A parcel of very good powder being placed on a white paper, which was pierced with a great number of fine holes, and the paper being held over the steam of hot water, I found, that in half a minute the powder was increased in weight by about $\frac{1}{50}$ part.

Trying another parcel in the same manner, but continuing it longer in the steam, I found that the powder increased its weight by $\frac{1}{24}$ part; but in this case some of the grains adhered together in small lumps, although the figure of the grains themselves was no ways changed.

To convince myself that the moisture of the atmosphere would likewise increase the weight of powder, I took about an ounce of powder, which had

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had for some time been kept in a room, which had a fire in it every day, and I found, by drying it before the fire, that it lost above $\frac{1}{150}$ of its weight; one third of which decrease in weight it had again acquired in less than two hours, by being removed to a different part of the room, at a distance from the fire.

Now as the weather is often much moister than when I tried this experiment, and as in open air this moisture abounds much more than in a room where there is a fire, it cannot be doubted, but that sometimes the twentieth or thirtieth part of the substance of the best powder is water, which may be easily supposed to produce all the effects, we have observed and described in this proposition.

But, however, the moisture thus imbibed by powder from the air does not, as I have yet observed, render it less active, when it is dried again. The reader must have observed, in the experiments of the last proposition, how nearly those made with the same quantities of powder, and in the same circumstances, agree with each other. In these experiments, though made at different times in the course of three summer months, the dryness of the season prevented all the inequalities of this proposition. But trying the same powder in the winter, in a very damp season, I found, that though if it was used as in the summer, in its natural state, without any drying, its effects were very irregular, and much short of those experiments; yet if each charge was well dried, just before it was used, no diminution of its force could then be perceived, nor did it appear to act in any manner different from what it had done in the preceding summer. Indeed if the powder be exposed to the greatest damps without any caution, or if common salt abounds in it, the moisture it imbibes may, perhaps, be sufficient to dissolve some part of the nitre; which is a lasting damage, that no drying can

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can retrieve. But when tolerable care is taken in preserving powder, and the nitre it is composed of has been well purged from common salt, it will retain its force much longer than is usually supposed. I have heard that powder which had been well kept, did not at the end of fifty years appear to be any ways injured by its age.

Some care is necessary in the drying of damp powder; for there is a degree of heat, which though not sufficient to fire the powder, will yet melt the brimstone, and destroy the texture of the grains.—Nay more, there is a heat, with which the brimstone will flame and burn away gradually, and yet the powder will not explode; of this any one may satisfy himself by heating a piece of iron red-hot, and then throwing a few grains of powder on it at different intervals, during the time of its cooling; for by this means he will find, that at a certain time the separate grains, that fall on the iron, will not explode, but will burn with a small blue flame for some space of time, the grain still remaining unconsumed. Indeed, when it has begun to burn in this manner, it sometimes ends with exploding, but this more commonly happens when a number of grains lie near together; for then, though each separate flame is not sufficient to explode its respective grain, yet the whole fire, made by them altogether, grows strong enough at last to end in a general explosion; however, by attending to the proper temperature of the iron, and spreading the grains, I have often covered two or three square inches with a blue lambent flame, which has lasted a considerable time without any explosion; and examining the grains afterwards, I could not perceive that they had lost either their colour or their shape. Now since these grains, when the brimstone is thus burnt, or even melted out of them, will no longer act as powder; it is evident, that

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powder may be spoiled by being dried with too violent a heat.

From the great difference in the effects of moist and dry powder established in this proposition, it appears how very uncertain and irregular all those practical operations of Gunnery may prove, where this circumstance is not attended to; and how little confidence can be placed in any experiments where this cause of inequality could interfere.

Before I leave this article, I must mention a suspicion, I once entertained about this matter.—As water, when rarefied into vapour, is generally supposed to be near ten times more elastic than air equally heated, I imagined, that possibly the moisture imbibed by power might, in certain cases, be so proportioned to the quantity of powder, that it might be converted into vapour by the explosion; and that thereby the force of the powder might be more increased by the addition of this very elastic vapour, than it was diminished by the damping of its flame. And I was the more induced to believe that this did sometimes happen, from the experiments of a late author, who tells us that the ranges of the same shot, fired from the same mortar, with equal charges of powder, were much greater in the freshness of the morning, than in the heat of the day. For I was well satisfied, that the mere density of the air (to which he seems to impute this variety) could not produce such different effects. However, upon a more accurate examination, I cannot find that any degree of moisture does at any time augment the force of powder; for, in all the numerous trials I have made, I never observed that force sensibly to exceed its mean quantity, except in two experiments; and even those excesses, I had good reason to believe, were occasioned by some disorder in the machine. However, if the elasticity of watery vapour be as great as it is usually esteemed, (a point far from being ascertained

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ascertained at present) it is not impossible, but something of this kind may take place in the firing of large quantities of powder.

PROP. XI.

To investigate the Velocity which the Flame of Gunpowder acquires, by expanding itself, supposing it be fired in a given Piece of Artillery, without either a Bullet or any other Body before it.

If the whole substance of the powder was converted into an elastic fluid at the instant of the explosion; then, from the known elasticity of this fluid assigned by our theory, and its known density, we could easily determine the velocity with which it would begin to expand, and could thence trace out its future augmentations in its progress through the barrel; but as we have shewn that the elastic fluid, in which the activity of the gunpowder consists, is only $\frac{3}{10}$ of the substance of the powder, the remaining $\frac{7}{10}$ will in the explosion be mixed with the elastic part, and will by its weight retard the activity of the explosion; and yet they will not be so completely united as to move with one common motion, but the unelastic part will be less accelerated than the rest, and some of it will not even be carried out of the barrel, as appears by the considerable quantity of unctuous matter which adheres to the inside of all fire-arms, after they have been used.

These inequalities in the expansive motion of the flame oblige us to recur to experiments for its accurate determination.

The experiments, made use of for this purpose, were of two kinds: the first was made by charging the barrel A with 12dw. of powder, and a small wad of tow only; and then placing its mouth 19 inches from the centre of the pendulum, mentioned

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in the seventh proposition; on firing it in this situation, the impulse of the flame on the pendulum made it ascend through an arch, whose chord was 13,7 inches; whence, if the whole substance of the powder was supposed to strike against the pendulum, and each part to strike with the same velocity, that common velocity must have been at the rate of about 2650 feet in 1″. This, then, is the least velocity, which the powder could be supposed to acquire in its expansion; for if we suppose the elastic part to acquire a greater velocity in expanding, than the other gross vapour, (which it undoubtedly does) this common velocity here assigned must be augmented for the elastic fluid, and diminished for the grosser substance of the powder. As some part of the velocity of the flame was lost in passing through 19 inches of air, I made the remaining experiments on this subject in a manner not liable to that inconvenience.

I fixed the barrel A on the pendulum, so that its axis might be both horizontal and also perpendicular to the plane HK; or, which is the same thing, that it might be in the plane of the pendulum's vibration; the height of the axis of the piece above the centre of the pendulum was six inches; and the weight of the piece, and of the iron that fastened it, &c. was 11lb. ½. The barrel in this situation being charged with 12dw. of powder, without either ball or wad, the powder only put together with the rammer, on the discharge the pendulum ascended through an arch, whose chord was 10 inches, or reduced to an equivalent blow in the centre of the pendulum, supposing the barrel away, it would be 14,4 inches nearly.

The same experiment repeated again, the chord of the ascending arch was 10,1 inches, which reduced to the centre, is 14,6 inches.

To determine what difference of velocity there was in the different parts of the vapour, I loaded the

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the piece again with 12dw. of powder, and rammed it down with a wad of tow weighing 1dw. Now I conceived, that this wad, being very light, would presently acquire that velocity, with which the elastic part of the fluid would expand itself when uncompressed; and I accordingly found that the chord of the ascending arch, was by this means augmented to 12 inches, or at the centre to 17,3: whence, as the medium of the other two experiments is 14,5, the pendulum ascended through an arch 2,8 inches longer, by the additional motion of 1dw. of matter moving with the velocity of the swiftest part of the vapour; and consequently, the velocity, with which this 1dw. of matter moved, was that of about 7000 feet in 1″.

It will, perhaps, be objected to this determination, that the augmentation of the arch, through which the pendulum vibrated in this case, was not all of it owing to the quantity of motion given to the wad, but part of it was produced by the confinement of the powder, and the greater quantity thereby fired. But if it were true that a part only of the powder fired, when there was no wad, it would not happen, that in firing different quantities of powder without a wad, the chord of the ascending arch would increase and decrease nearly in the ratio of those quantities, which yet I have found it to do; for with 9dw. that chord was 7,3 inches, which with 12dw. we have seen was but 10, and 10,1; and even with 8dw. the chord was 2 inches; deficient from this proportion by ,5 only; for which defect too, other valid reasons are to be assigned.

And there is still a more convincing proof, that all the powder is fired, although no wad be placed before the charge; which is, that the part of the recoil arising from the expansion of the powder alone, is found to be no greater when it impels a leaden bullet before it, than when the same quantity

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tity is fired without any wad to confine it. We have seen that the chord of the arch, through which the pendulum rose from the expansive force of the powder alone, is 10, or 10,1 ; and the chord of that arch when the piece was charged in the customary manner with a bullet and wad, I found to be the first time $22\frac{1}{4}$, and the second time $22\frac{7}{8}$; or at a medium 22,56. Now the impulse of the ball and wad, if they were supposed to strike the pendulum in the same place in which the barrel was suspended, with the velocity they had acquired at the mouth of the piece, would drive it through an arch whose chord would be about 12,3, as is known from the weight of the pendulum, the weight and position of the barrel, and the velocity of the bullet, determined by our former experiments; whence, subtracting this number 12,3 from 22,56, the remainder 10,26, is nearly the chord of the arch, which the pendulum would have ascended through, from the expansion of the powder alone, when fired with a bullet before it; and this number 10,26 differs very little from 10,1, which we have above found to be the chord of the ascending arch, when the same quantity of powder expanded itself freely, without either bullet or wad before it.

Again, that this velocity of 7000 feet in 1″ is not much beyond what the most active part of the flame acquires in expanding, is evinced from hence, that we have above, in the 38th experiment, an instance of a ball actually discharged with a velocity of 2400 in 1″, and yet it appeared not that the action of the powder on this bullet, was at all diminished on account of this immense celerity; consequently, the degree of swiftness with which in this instance the powder followed the ball, without losing any part of its pressure, must have been much short of what the powder alone would have expanded with, had not the ball been there.

And it is this prodigious celerity of expansion of

of the flame of fired powder, that is its peculiar excellence; and the circumstance in that it so eminently surpasses all other inventions, either ancient or modern, for the purpose of military projections: for as to the quantity of motion of these projectiles only, many of the warlike machines of the ancients produced this in a degree far surpassing that of our heaviest cannon-shot or shells; but the great celerity given to these bodies, cannot be in the least approached by any other means than by the flame of powder. The reason of this difference is, that the ancients could by weights, or the elasticity of springs and stretched cords, augment their powers to any degree desired; but then each addition of power brought with it a proportional addition of matter to be moved; so that as the power increased, these parts of the machine which were to communicate motion to the projectile, and were consequently to move with it, were likewise increased; and thence it necessarily happened, that the action of the power was not solely employed in giving motion to the impelled body, but much the greatest part of it was spent in accelerating those parts of the machine in which the power resided, to enable them to pursue the body to be projected with a perpetual impulse, during its whole passage through the extent of their activity. Hence then it came to pass, that though these ancient machines could throw enormous weights, they could project them but with small degrees of celerity, compared with what we can communicate to our cannon and musket-shot: whence, in all operations, where these great velocities are useful, our machines are infinitely superior to those of antiquity: although in more confined and shorter projections, these last have some advantages, which may yet render them worthy of the attention of those military geniuses, who have capacity enough to consider each part of their profession according

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to its true and genuine value, independent of the partial estimation of the times they live in.

From the determinations contained in this proposition, the force of petards may be deduced, since their action solely depends on the impulse of the flame: and it appears, that a quantity of powder, properly disposed in such a machine, may produce as violent an effort as a bullet of twice its weight, moving with a velocity of 14 or 1500 feet in 1".

PROP. XII.

To ascertain the Manner in which the Flame of Powder impels a Ball, which is laid at a considerable Distance from the Charge.

We have, in many of the experiments recited by us above, laid the ball not immediately contiguous to the powder, but a small distance from it; the greatest interval, however, has not amounted to more than about $\frac{1}{4}$ inch, from the hinder part of the bullet to the nearest part of the powder; and, in these cases, we have seen, that the theory agreed very well with the experiments: but if a bullet be placed at a greater distance from the powder, suppose at 12, 18, or 24 inches, we cannot then apply to the motion of this ball the same principles which, in the 7th proposition, we have applied to such as are contiguous to the powder, or nearly so; for we have seen, in the last proposition, that, when the surface of the fired powder is not confined by a heavy body, which it is obliged to impel before it, the flame dilates itself with a velocity much beyond what it can at any time communicate to a bullet by its continued pressure; consequently, as in the distance of 12, 18, or 24 inches, the powder will have acquired a considerable degree of this velocity of expansion, the first motion of the ball will not be produced by the

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continued pressure of the powder, but by the actual percussion of the flame; and it will therefore begin to move with a quantity of motion proportioned to the quantity of this flame, and the velocities of its respective parts.

From hence then it follows, that the velocity of a bullet, laid a considerable distance before the charge, ought to be greater, than what would be communicated to it by the pressure of the powder acting in the manner described in the 7th proposition; and this deduction from our theory we have confirmed by manifold experience; by which we have found, that a ball laid in the barrel A, with its hinder part 1 $\frac{1}{4}$ inches from its breech, and impelled by 12dw. of powder, has acquired on its discharge a velocity of about 1400 feet in 1″; when, if it had been acted on by the pressure of the flame only, it would not have acquired a velocity of 1200 feet in 1″. The same we have found to hold true in all other greater distances, (and also in lesser, though not to the same degree) and in all quantities of powder. And we have likewise found, that these effects nearly correspond with what was laid down in the last proposition about the velocity of expansion, and the elastic and unelastic parts of the flame.

And from hence too arises another consideration of great consequence in the practice of Gunnery; which is, that no bullet should at any time be placed at any considerable distance before the charge, unless the piece be extremely fortified; for a moderate charge of powder, when it has expanded itself through the vacant space, and reaches the ball, will, by the velocity each part has acquired, accumulate itself behind the ball, and will thereby be condensed prodigiously; whence, if the barrel be not of an extraordinary firmness in that part, it must by this reinforced elasticity of the powder, infallably burst. The truth of this

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reasoning I have experienced in an exceeding good *Tower*-musquet, forged of very tough iron; for charging it with 12dw. of powder, and placing the ball 16 inches from the breech, on the firing it, the part of the barrel just behind the bullet was swelled out to double its diameter, like a blown bladder, and two large pieces, of two inches long, were burst out of it.

Having seen that the entire motion of a bullet, laid at a considerable distance from the charge, is acquired by two different methods, in which the powder acts on it; the first being the percussion of the parts of the flame, with the velocity they had respectively acquired by expanding; the second, the continued pressure of the flame through the remaining part of the barrel; I endeavoured to separate these different actions, and to retain that only, which arose from the continued pressure of the flame. For this purpose, I no longer placed the powder at the breech, from whence it would have full scope for its expansion, but I scattered it as uniformly as I could, through the whole cavity left behind the bullet; imagining that, by this means, the progressive velocity of the flame in each part would be prevented by the expansion of the neighbouring parts: and I found, that the ball being laid $11\frac{1}{4}$ inches from the breech, its velocity, instead of 1400 feet in 1″, which it acquired in the last experiments, was now no more than 1100 feet in 1″; which is 100 feet short of what, according to the theory, should arise from the continued pressure of the powder only.

The reason of this deficiency was, doubtless, the intestine motion of the flame; for the accension of the powder, thus distributed through so much a larger space than what it could fill, must have produced many reverberations and pulsations of the flame; and from these internal agitations of the fluid, its pressure on the containing surface will

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will (as is the case in all other fluids) be considerably diminished; and it has been in order to avoid this irregularity, that in all the experiments I have made, I have taken particular care to have the powder closely confined in as small a space as possible, even when the bullet lay at some little distance from it.

PROP. XIII.

To enumerate the various Kinds of Powder, and to describe the properest Methods of examining its Goodness.

THE powder, we have hitherto considered, is supposed to be such as is made for the service of the government; but, besides this, there are many other kinds, some better and some worse, which I here propose to enumerate, as far as they have come to my knowledge.

But, first, I must premise, that the government powder, if properly wrought, is, I believe, nearly as good as any powder made for general use. I have examined it with great care, and have compared it with other powders made here in *England*, which are esteemed the best, such as the *Battle*, &c. and I cannot find any sensible difference between them. I have likewise compared it, in frequent trials, with some *Spanish* powder, taken out of the *St. Jago* prize; and though, if I were to give my opinion, I should rather believe the *Spanish* powder the better of the two, yet so small an inequality as a fiftieth or sixtieth part, which is the most, that the difference between them can amount to, is too little to be ascertained with absolute certainty. I conceive too, by comparing the experiments of others with my own, that the *French* powder is little different from ours; although I cannot be so certain on this head as I could wish, having never been able to procure any

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of their powder myself. But it must be remembered, that when I speak of our government powder, it must be what is supposed to be made of the standard proportion of materials, and properly wrought; for such was the powder I made use of.

The strongest powder, I have yet met with, is some which I am told was made in *Holland;* its force, compared with that of our government powder, is nearly as 5 to 4. But this powder is undoubtedly made of the choicest picked materials, and is probably wrought up with spirits; so that quantities of it could not be made, but at a much greater expence, than what would be repaid by its additional strength.

The next best powder, that has come to my hands, is a powder made in *Portugal*, under the direction of a *Dutchman*, who some years since established powder-mills near *Lisbon*. This is in strength inferior to the *Dutch* powder last mentioned; but is however nearer to that than to our government powder.

The common sale powder here in *England*, such as is to be had at every grocer's, is much worse than the government or the *battle* powder, and extremely various, according to the caprice of the maker. I have tried some, whose strength has been in proportion to the government powder, as 2 to 3 nearly, and other parcels have been still worse; but the worst of all is the powder made for the *African* trade, usually styled *Guinea* powder: but these weaker powders are not worth examination, as there is no established standard for their composition.

Now these differences in the strength of powder may arise from three causes; either from the quality of the materials, from the proportion observed in their mixture, or from the manner of working them together.

Powder, as is generally known, is composed of saltpetre,

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saltpetre, sulphur, and charcoal: of these materials the sulphur and charcoal are much the cheapest; and though there are peculiar kinds of these, which are fittest for this purpose, yet the expence of having the very best is so small, when compared to the whole cost of the powder, that it is strange if powder, which would be otherwise good, is spoiled by bad sulphur or charcoal.

The most expensive part of the composition, and consequently the part to which the defects of powder are oftner owing, is the saltpetre. This is a substance imbibed by the earth from the air; for a quantity of earth, which has had its saltpetre washed out of it, will, when it has been exposed to the air for some time, produce saltpetre again; and this as often as the experiment shall be repeated.

Saltpetre is of itself an uninflammable substance; for if it be placed in the most violent fire, it only melts, and never flames, provided no combustible matter is permitted to mix with it: but though of itself, unmixed with other bodies, it will neither flame nor burn; yet, if it be joined with burning substances, it prodigiously augments the violence of their burning; performing, in this case, what the air, forcibly mixed with fire by the blast of a pair of bellows, does in a much inferior degree.

Powder then being a mixture of sulphur and charcoal, which are very inflammable substances, with saltpetre, which in itself is not, if the saltpetre be too much in quantity, when compared with the other two, their burning may not be sufficient to consume the whole of the saltpetre; whence the fire may be less violent, and consequently (according to what we have observed in the 10th proposition) the powder less vigorous, than if some of the saltpetre was taken away, and a like quantity of the other materials were added in its stead. On the other hand, if the saltpetre

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in the composition be less than what the burning of the other two substances can easily consume, the fire will be less active than it ought to be; because it is not augmented so much as it would be, if a larger quantity of saltpetre had been added to the composition.

Hence then it appears, that the goodness of powder is not to be estimated only from the quantity of saltpetre contained in it, although that substance seems to be the basis of the elastic fluid, in which its force consists; for since the converting of the saltpetre into that fluid, and the elasticity of the fluid afterwards, depend in some measure on the violence of the fire produced at the explosion, it is plain that there is a certain proportion in the mixture of the materials, which will best contribute to this purpose, and consequently to the perfection of the powder.

What this proportion is, has been ascertained by experience; and it seems now to be generally agreed, that in any quantity of powder $\frac{3}{4}$ of it should be saltpetre, the remaining $\frac{1}{4}$ consisting of equal quantities of sulphur and charcoal. This is the proportion followed by the *French*, and I believe by most nations in *Europe*; we indeed pretend to a greater degree of nicety in our proportions; though I am told they do not greatly differ from what I have mentioned; nor am I convinced that they are preferable: this I am sure of, that no methods of proving powder, hitherto generally practised in *England*, could at all ascertain the difference; and other powders, made with the usual proportions, are no whit inferior to ours.

But it is not the due proportion of the materials only, which is necessary to the making of good powder; another circumstance, not less essential, is the mixing them well together; if this be not effectually done, some parts of the composition will have too much saltpetre in them, and others too little;

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little; and in either case there will be a loss of strength in the powder.

As the excellency of powder then depends on so many particulars, in the quality and quantity of the materials, and in the working them together; it is doubtless of great importance, that those who receive the public stores should have it in their power to satisfy themselves about the goodness of what is delivered to them. The method most commonly followed for this purpose, here with us, is (if I am rightly informed) to fire a small heap of it on a clean board, and to attend nicely to the flame and smoke it produces, as likewise to the marks it leaves behind it on the table; from all which instructive particulars the merit of the powder is ascertained with great accuracy, as is pretended: but besides this uncertain method, which I presume (how much soever it may be practised) none will seriously undertake to defend, there are, on particular occasions, other contrivances made use of; all which bear some analogy to the common powder-triers, sold at the shops; only they are more artfully fabricated, and instead of a spring they move a weight, which is a more certain and equable power.

But these machines, though more perfect than the common powder-triers, are yet liable to great irregularities; for as they are all moved by the instantaneous stroke of the flame, and not by its continued pressure, they do not determine the force of the fired powder with that certainty and uniformity, which were to be desired in these kinds of trials: and therefore I cannot but think the method followed by the *French*, in the receiving of powders from the makers, to be much better. Their practice is thus.

They have, in each magazine, a small mortar cast, with its bed, according to a determined pattern, which is the same throughout the kingdom: this

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this mortar is always pointed at 45°, and it contains just three ounces of powder; and it is a standing maxim, that no powder can be received into their stores, unless three ounces of it, placed in the chamber of this mortar, throws a solid ball, of 7½ inches diameter, to the distance of at least 55 *French* fathom.

It has been objected to this method, that if each barrel of powder was to be proved in this manner, the trouble of charging the mortar, and bringing back the ball each time, would be intolerable, and the delay so great, that no business of this kind could ever be finished; and if a number of barrels are received on the merit of a few, it is great odds but some bad ones will be amongst them, which may prove a great disappointment in time of service. Add to this another exception, which to me has much more weight; and that is, the monstrous disproportion between the weight of the ball and the powder that projects it; so that the powder continues in action a longer time, and expands through a much larger space, in proportion to its quantities in these trials, than it ever does in any real service; whence it happens, that the vapour cools, and great part of it escapes through the touch-hole, or by the side of the bullet; so that the quantity of motion produced by the explosion is, in this instance, but little more than half of what it ought to be, if the powder acted on the ball with its full force undiminished by these accidents; consequently, as this diminution of force may not be always constant, the action of the same powder, by the varying of these adventitious circumstances, may, at different times, convey the ball to different distances.

Now this last exception does no ways hold against the method by which I have tried the comparative strength of different kinds of powder, which has been by the actual velocity given to a bullet,

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bullet, by such a quantity of powder as is usually esteemed a proper charge for the piece: and as this velocity, however great, is easily discovered by the motion, which the pendulum acquires from the stroke of the bullet, (according to the principles laid down above) it might seem a good amendment to the method used by the *French*, to introduce this trial by the pendulum instead of it. But though I am satisfied, that this would be much more accurate, less laborious, and readier than the other, yet, as there is some little attention and caution required in this practice, which might render it of less dispatch than might be convenient, when a great number of barrels were to be separately tried, I should myself chuse to practise another method not less certain, but prodigiously more expeditious; so that I could engage, that the weighing out of a small parcel of powder from each barrel should be the greatest part of the labour; and, doubtless, three or four hands could, by this means, examine 500 barrels in a morning; besides, the machines for this purpose, as they might be made of cast iron, would be so very cheap, that they might be multiplied at pleasure. However, I shall defer the description of this method at present, and shall proceed to the consideration of the resistance of the air, a subject of the greatest importance to the perfection of gunnery.*

CHAP.

* Other eprouvettes have been since devised, which will be noticed hereafter. H.

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CHAP. II.

Of the Resistance of the Air, and of the Track described by the Flight of Shot and Shells.

BEFORE I more minutely discuss the subject of this chapter, it is necessary to premise, that the greatest part of authors have established it as a certain rule, that whilst the same body moves in the same medium, it is always resisted in the duplicate proportion of its velocity; that is, if the resisted body move in one part of its track with three times the velocity, with which it moved in some other part, then its resistance to the greater velocity will be nine times the resistance to the lesser. If the velocity in one place be four times the velocity in another, the resistance to the greater velocity will be sixteen times the resistance to the lesser, and so on. This rule, though excessively erroneous, (as we shall hereafter shew) when taken in a general sense, is yet undoubtedly very near the truth, when confined within certain limits; and therefore, in our future dispositions, we shall suppose, that in all small changes of velocity in the resisting body, it does accurately hold true; so that when we speak hereafter of the resistance of the medium being increased or diminished by the varying of the velocity, we shall not hereby include that increase or diminution, which ought to take place according to this law, but shall thereby intend a resistance, greater or less than what the moving body ought to undergo from the application of this principle; that is, we shall thereby understand an increase or diminution in the resisting power of the medium, similar to what might be occasioned

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occasioned by increasing or diminishing its density: the principal purport of our present attempt being to evince, that according to the different compression of the medium, or the different degree of velocity in the moving body, such changes may arise in the resisting power of the medium, as could scarcely be effected, according to the principles commonly received on this subject, by a treble augmentation of its density. This we doubt not irrefragably to confirm in the following dissertation.

PROP. I.

To describe the general principles of the Resistance of Fluids to solid Bodies moving in them.

In order to conceive the resistance of fluids to a body moving in them, it is necessary to distinguish between those fluids, which being compressed by some incumbent weight, perpetually close up the space deserted by the body in motion, without permitting for an instant any vacuity to remain behind it; and those fluids in which (they being not sufficiently compressed) the space left behind the moving body remains for some time empty. These differences, in the resisting fluids, will occasion very remarkable varieties in the laws of their resistance, and are absolutely necessary to be considered in the determination of the action of the air on shot or shells; for the air partakes of both these affections, according to the different velocities of the projected body.

If a fluid was so constituted, that all the particles composing it were at some distance from each other, and there was no action between them; then the resistance of a body moving therein would be easily computed, from the quantity of motion communicated to these particles: for instance, if a cylinder

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linder moved in such a fluid in the direction of its axis, it would communicate to the particles it met with a velocity equal to its own, and in its own direction, supposing that neither the cylinder, nor the parts of the fluid, were elastic; whence, if the velocity and diameter of the cylinder be known, and also the density of the fluid, there would thence be determined the quantity of motion communicated to the fluid, which (action and re-action being equal) is the same with the quantity lost by the cylinder, consequently the resistance would be hereby ascertained.

In this kind of discontinued fluid, the particles being detached from each other, every one of them can pursue its own motion in any direction, at least for some time, independent of the neighbouring ones; wherefore, if, instead of a cylinder moving in the direction of its axis, a body, with a surface oblique to its direction, be supposed to move in such a fluid, the motion the parts of the fluid will hereby acquire, will not be in the direction of the resisted body, but perpendicular to its oblique surface; whence the resistance to such a body will not be estimated from the whole motion communicated to the particles of the fluid, but from that part of it only, which is in the direction of the resisted body. In fluids then, where the parts are thus discontinued from each other, the different obliquities of that surface, which goes foremost, will occasion considerable changes in the resistance, although the section of the solid by a plain perpendicular to its direction should in all cases be the same. And Sir *Isaac Newton* has particularly determined, that in a fluid thus constituted, the resistance of a globe is but half the resistance of a cylinder of the same diameter, moving in the direction of its axis with the same velocity.

But though the hypothesis of a fluid, thus constituted, be of great use in explaining the nature of resistances;

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resistances; yet, in reality, no such fluid does exist within our knowledge: all the fluids, with which we are conversant, are so formed, that their particles either lie contiguous to each other, or at least act on each other in the same manner as if they did; consequently, in these fluids, no one particle, contiguous to the resisted body, can be moved, without moving at the same time a great number of others, some of which will be distant from it; and the motion thus communicated to a mass of the fluid, will not be in any one determined direction, but will in each particle be different, according to the different manner in which it lies in contact with those from whence it receives its impulse; whence, great numbers of the particles being diverted into oblique directions, the resistance of the moving body, which will depend on the quantity of motion communicated to the fluid in its own direction, will be hereby different in quantity, from what it would be in the preceding supposition, and its estimation becomes much more complicated and operose.

If the fluid be compressed by the incumbent weight of its upper parts (as all fluids are with us, except at their very surface) and if the velocity of the moving body be much less than that with which the parts of the fluid would rush into a void space, in consequence of their compression, it is evident, that in this case the space left by the moving body will be instantaneously filled up by the fluid, and the parts of the fluid against which the foremost part of the body presses in its motion, will, instead of being impelled forwards in the direction of the body, circulate in some measure towards the hinder part of the body, thereby to restore the equilibrium, which the constant influx of the fluid behind the body would otherwise destroy; whence the progressive motion of the fluid, and consequently the resistance of the body, which

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which depends thereon, would be in this instance much less than in our first hypothesis, where each particle was supposed to acquire, from the stroke of the existing body, a velocity equal to that with which the body moved, and in the same direction. Sir *Isaac Newton* has determined, that the resistance to a cylinder moving in the direction of its axis, in such a compressed fluid, as we have here treated of, is but one fourth part of the resistance, which the same cylinder would undergo, if it moved with the same velocity in a fluid constituted in the manner we have described in our first hypothesis, each fluid being supposed to be of the same density.

But again, it is not only in the quantity of their resistance that these fluids differ, but likewise in the different manner in which they act on solids of different forms moving in them.

We have shewn, that in the discontinued fluid, which we first described, the obliquity of the foremost surface of the moving body would diminish the resistance; but in compressed fluids this holds not true, at least not in any considerable degree; for the principal resistance in compressed fluids arises from the greater or lesser facility, with which the fluid, impelled by the fore part of the body, can circulate towards its hindermost part; and this being little, if at all, affected by the form of the moving body, whether it be cylindrical, conical, or spherical, it follows, that while the transverse section of the body, and consequently the quantity of impelled fluid be the same, the change of its figure will scarcely affect the quantity of its resistance.

And this case, that is, the resistance of a compressed fluid to a solid, moving in it with a velocity much less than what the parts of the fluid would acquire from their compression: this case, I say, has been very fully considered by Sir *Isaac Newton*,

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Newton, who has ascertained the quantity of such a resistance according to the different magnitudes of the moving body, and the density of the fluid. But he very expressly informs us, that the rules he has laid down are not generally true, but upon a supposition that the compression of the fluid be increased in the greater velocities of the moving body: however, some unskilful writers who have followed him, overlooking this caution, have applied his determinations, to bodies moving with all kinds of velocities, without attending to the different compressions of the fluids they were resisted by; and by this means they have accounted the resistance of the air to musket and cannon-shot to be but one third part, of what I have found it by experience.

Indeed, from all we have said, it appears plain enough, that the resisting power of the medium must be increased, when the resisting body moves so fast, that the fluid cannot instantaneously press in behind it, and fill the deserted space; for when this happens, the body will be deprived of the pressure of the fluid behind it, which in some measure ballances its resistance, and must support on its fore part the whole weight of a column of the fluid, independent of the motion it gives to the parts of the fluid; and besides, the motion in the particles driven before the body is, in this case, less affected by the compression of the fluid, and consequently they are less deflected from the direction, in which they are impelled by the resisted surface; whence this species of resistance approaches more and more to that described in our first hypothesis, where each particle of the fluid being unconnected with the neighbouring ones, pursued its own motion, in its own direction, without being interrupted or deflected by their contiguity; and therefore, as we before observed, that the resistance of a discontinued fluid to a cylin-

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der, moving in the direction of its axis, was four times greater than the resistance of a fluid sufficiently compressed of the same density, it follows, that the resistance of a fluid, when a vacuity is left behind the moving body, may be near four times greater than that of the same fluid, when no such vacuity is formed; for when a void space is thus left, we have shewn the resistance to approach in its nature to that of a discontinued fluid.

This then may probably be the case in a cylinder moving in the same compressed fluid, according to the different degrees of its velocity; so that if it set out with a great velocity, and moves in the fluid till that velocity be much diminished, the resisting power of the medium may be near four times greater in the beginning of its motion than in the end. In a globe the difference will not be so great, because on account of its oblique surface, its resistance in a discontinued medium is but about twice as much as in one properly compressed; for its oblique surface diminishes its resistance in one case and not in the other: however, as the compression of the medium, even when a vacuity is left behind the moving body, may yet confine the oblique motion of the parts of the fluid, which are driven before the body, and as in an elastic fluid (as is our air) there will be some degree of condensation in those parts, it is highly probable, that the resistance of a globe, moving in a compressed fluid with a very great velocity, will be between that of a globe and of a cylinder, in a discontinued medium; that is, (in proportion to its velocity) we may suppose it to be more than twice, and less than four times the resistance of the same globe, moving slowly through the same medium; whence, perhaps, we shall not much err in supposing the globe in its swiftest motions to be resisted near three times more, in proportion to its velocity, than when it is slowest.

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And as this increase of the resisting power of the medium will take place, when the velocity of the moving body is so great, that a perfect vacuity is left behind it, so some degree of augmentation will be sensible in velocities much short of this; for even when, by the compression of the fluid, the space left behind the body is instantaneously filled up, yet if the velocity, with which the parts of the fluid rush in behind, is not much greater than that, with which the body moves, the same reasons we have urged above, in the case of an absolute vacuity, will hold in a less degree in this instance; and therefore we are not to suppose, that the increased resistance, which we have hitherto treated of, immediately vanishes, when the compression of the fluid is just sufficient to prevent a *vacuum* behind the resisted body; but we must consider it as diminishing only, according as the velocity, with which the parts of the fluid follow the body, exceeds that, with which the body moves.

Hence then we conclude, that if a globe sets out in a resisting medium, with a velocity much exceeding that with which the particles of the medium would rush into a void space, in consequence of their compression, so that a *vacuum* is necessarily left behind the globe in its motion, the resistance of this medium to the globe will be near three times greater, in proportion to its velocity, than what we are sure, from Sir *Isaac Newton*, would take place in a slower motion. We may farther conclude, that the resisting power of the medium will gradually diminish, as the velocity of the globe decreases, till at last, when it moves with velocities, which bear but a small proportion to that, with which the particles of the medium follow it, the resistance becomes the same with what is assigned by Sir *Isaac Newton* in the case of a compressed fluid.

And from this determination we may learn, how

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false that position is, which asserts the resistance of any medium to be in the duplicate proportion of the velocity of the resisted body; for it plainly appears, by what we have said, that this can only be considered as nearly true in some variations of velocity, and can never be applied in the comparing together the resistances to all velocities whatever without the most enormous errors.

These principles being laid down, we shall next proceed to an experimental examination of the resistance of the air in particular, both in order thence to evince how nearly these speculations agree to the real observed action of fluids, and likewise to shew, how egregiously all those theorists have been mistaken, who have conceived, that the resistance of the air to shells and shot of all kinds was scarcely worthy of attention.

PROP. II.

To determine the resistance of the air to projectiles by experiments.

By means of the machine described in the 8th proposition, I have it in my power to determine the velocity, with which a ball moves in any part of its track, provided I can direct the piece so as to cause the bullet to impinge on the pendulum placed in that part; and therefore charging a musket-barrel three times successively with a leaden ball of $\frac{3}{4}$ of an inch diameter, and about half its weight of powder, and taking such precaution in the weighing of the powder, and placing it, that I was assured, by many previous trials, that the velocity of the ball could not differ by 20 feet in 1″ from its medium quantity, I fired it against the pendulum placed at 25 feet, at 75 feet, and at 125 feet distance from the mouth of the piece respectively; and I found that it impinged against the pendulum in the first case with a velocity of 1670 feet

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feet in 1″, in the second case with a velocity of 1550 feet in 1″, and in the third case with a velocity of 1425 feet in 1″; so that in passing through 50 feet of air, the bullet lost a velocity of about 120 or 125 feet in 1″; and the time of its passing through that space being about $\frac{1}{11}$ or $\frac{1}{10}$ of 1″, the medium quantity of resistance must, in these instances, have been about 120 times the weight of the ball, which (as the ball was nearly $\frac{1}{12}$ of a pound) amounts to about 10lb. avoirdupois.

Now if a computation be made according to the method laid down for compressed fluids in the 38th proposition, lib. 2. of Sir *Isaac Newton's Principia*, supposing the weight of water to be to the weight of air, as 850 to 1, it will be found, that the resistance to a globe of $\frac{3}{4}$ of an inch diameter, moving with a velocity of about 1600 feet in 1″, will not, on those principles, amount to any more than a force of $4\frac{1}{6}$lb. avoirdupois; whence, as we know, that the rules contained in that proposition are very accurate in slow motions, we may hence conclude, that the resisting power of the air in slow motions is less than in swift motions in the ratio of $4\frac{1}{6}$ to 10, a proportion between that of 1 to 2 and 1 to 3.

Again, I charged the same piece, a number of times, with equal quantities of powder, and balls of the same weight, taking all possible care to give to every shot an equal velocity; and firing three times against the pendulum, placed 25 feet only distant from the mouth of the piece, the medium of the velocities with which the ball impinged, was nearly that of 1690 feet in 1″: then removing the piece 175 feet from the pendulum, I found, taking the medium of five shots, that the velocity with which the ball impinged, at this distance, was that of 1300 feet in 1″; whence the ball, in passing through 150 feet of air, lost a velocity of about 390 feet in 1″; and the resist-

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ance computed from these numbers comes out something more than in the preceding instance, it amounting here to between 11 and 12 pounds, avoirdupois; whence, according to these experiments, the resisting power of the air to swift motions is greater than in slow ones in a ratio, which approaches nearer to the ratio of 3 to 1, than in the preceding experiments.

Having thus ascertained the resistance to a velocity of nearly 1700 feet in 1″, which must be allowed to be more than sufficient for leaving a *vacuum* behind the ball, I next examined the resistance to smaller velocities; and for this purpose I charged the same barrel with balls of the same diameter, but with less powder; and placing the pendulum at 25 feet distance from the piece, I fired against it five times with an equal charge each time; the medium velocity, with which the ball impinged, was that of 1180 feet in 1″; then removing the pendulum to the distance of 250 feet, the medium velocity of five shots made at this distance was that of 950 feet in 1″; whence the ball, in passing through 225 feet of air, lost a velocity of 230 feet in 1″; and as it passed through that interval in about $\frac{3}{14}$ of 1″, the resistance to the middle velocity will come out to be near $33\frac{1}{2}$ times the gravity of the ball, or 2lb. 10oz. avoirdupois. Now the resistance to the same velocity, according to the laws observed in slower motions, amounts to $\frac{3}{14}$ of the same quantity; whence, in a velocity of 1065 feet in 1″, the resisting power of the air is augmented in no greater a proportion than that of 7 to 11; whereas we have seen, in the former experiments, that to still greater degrees of velocity, the augmentation approached very near to the ratio of 1 to 3.

But farther I fired three shot, of the same size and weight with those already mentioned, over a large piece of water; so that their dropping into the

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the water being very discernible, both the distance and time of their flight might be accurately ascertained; each shot was discharged with a velocity of 400 feet in 1″; and I had satisfied myself, by many previous trials of the same charge with the pendulum, that I could rely on this velocity to 10 feet in 1″. The first shot flew 313 yards in $4''\frac{1}{4}$, the second flew 319 yards in 4″, and the third 373 yards in $5''\frac{1}{4}$. According to the theory of resistance established for slow motions, the first shot ought to have spent no more than 3″,2,* in its flight, the second 3″,28, and the third 4″; whence it is evident, that every shot was retarded considerably more than it ought to have been, had that theory taken place in its motion: consequently, the resisting power of the air is very sensibly increased, even in so small a velocity as that of 400 feet in 1″.

From all that we have related then, it appears, that the theory of the resistance of the air, established in slow motions by Sir *Isaac Newton*, and confirmed by many experiments, is altogether erroneous, when applied to the swifter motions of musket or cannon-shot; for that, in these cases, the resisting power of the medium is augmented to near three † times the quantity assigned by that

 theory;

* The force being inversely as the square of the time, and $3{,}2^2 : 4{,}25^2 :: 10 : 17.6$; therefore the increase is here from 10 to 17,6, or from 1 to 1,76 or $1\frac{1}{4}$ nearly: that is, to the velocity of 313 yards or 939 feet per second, the actual resistance was about $1\frac{1}{4}$ of that computed by the square of the velocity. And this nearly agrees with what I have found by experiment with cannon balls; as may be perceived in page 365, vol. ii. of my Philosophical Dictionary. H.

† The increase to three times the quantity is certainly too great, both according to Mr. Robins's foregoing experiments, and to my own, made with cannon-balls. Indeed, Mr. Robins's are not exactly conformable to each other, made at different times, with velocities either equal or unequal, and thus manifesting some degree of inaccuracy in the experiments, as might be expected

 from

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theory; that, however, this increased power of resistance deminishes as the velocity of the resisted body diminishes, till at length, when the motion is sufficiently abated, the actual resistance coincides with that supposed in the theory; that therefore the resistance is not in the duplicate proportion of the velocity of the moving body, as is usually asserted, but varies from that proportion according to the different compression of the fluid compared with the velocity; consequently, from the consideration of these particulars, we may venture to assert, that whilst the resistance of the air was thus imperfectly and faultily conceived, the track of musket and cannon shot through that medium could not be ascertained with the least degree of certainty; and therefore the art of gunnery could not but continue extremely imperfect: however, it is not sufficient to have shewn the resistance to be augmented in great velocities, beyond what has been usually supposed; but, that we may be enabled more definitely to compute the motion of projectiles, it is necessary that we should assign the rate of this augmentation according to the different velocities of the resisted body. This shall be the subject of our next proposition.

PROP. III.

To assign the different augmentations of the resisting power of the air according to the different velocities of the resisted body.

As no large shot are ever projected in practice with velocities exceeding that of 1700 feet in 1″, I have

from his manner of making them with only musket balls, and with machinery less perfect than mine, made with cannon balls. However, the greatest increase that Mr. Robins found, in page 133, is only $4\frac{1}{6}$ to 10, or 1 to $2\frac{2}{5}$, considerably short of 1 to 3; whereas the greatest increase in my experiments, is rather short of 1 to $2\frac{1}{10}$. H.

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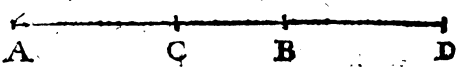
I have not as yet made any experiments on the resistance of bodies which have moved with a swifter motion than this, esteeming the determination of the variation of the resistance to all lesser velocities, to be sufficient for the purposes of this treatise.

According to the trials I have made, the resisting power of the air to velocities less than that of 1700 feet in 1″, may be thus nearly exhibited:



Let AB be taken to AC in the ratio of the velocity of 1700 feet in 1″, to the given velocity to which the resisting power of the air is required; continue the line AB to D, so that BD may be to AD, as the resisting power of the air to slow motions is to its resisting power to a velocity of 1700 feet in 1″, then shall CD be to AD, as the resisting power of the air to slow motions, is to its resisting power to the given velocity represented by AC.*

PROP. IV.

To determine the velocities with which musket and cannon-shot are discharged from their respective pieces by their usual allotment of powder.

From the computations of the 7th proposition of the 1st chapter, confirmed by the succeeding experiments, it plainly appears, that a leaden ball of $\frac{3}{4}$ of an inch in diameter, and weighing nearly $1\frac{1}{4}$oz. avoirdupois, if it be fired from a barrel of 45 inches in length, with half its weight of powder, will issue from that piece with a velocity which, if

* Instead of this rule, if we employ the actual resistance set down for every degree of velocity, in my general table quoted in the two preceding notes, we shall come nearer the truth. H.

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if it were uniformly continued, would carry it near 1700 feet in 1″.

If instead of a leaden ball, an iron one of the same diameter was placed in the same situation in the same piece, and was impelled by the same quantity of powder, the velocity of such an iron bullet would be greater than that of the leaden one, in the subduplicate ratio of the specific gravities of lead and iron; and supposing that ratio to be as 3 to 2, and computing on the principles laid down in the last-cited proposition, it will appear, that an iron bullet of 24lb. weight, shot from a piece of 10 feet in length, with 16lb. of powder, will acquire from the explosion a velocity which, if uniformly continued, would carry it nearly 1650 feet in 1″.

This is the velocity which, according to our theory, a cannon-ball of 24lb. weight is discharged with, when it is impelled by a full charge of powder; but if, instead of a quantity of powder weighing two-thirds of the ball, we suppose the charge to be only half the weight of the ball, then its velocity will, on the same principles, be no more than at the rate of 1490 feet in 1″; and the same would be the velocities of every lesser bullet, fired with the same proportions of powder, if the lengths of all pieces were constantly in the same ratio with the diameters of their bore: and although, according to the usual dimensions of the smaller pieces of artillery, this proportion does not always hold, yet the difference is not considerable enough to occasion a very great variation from the velocities here assigned; as will be obvious to any one, who shall make a computation thereon.

But in these determinations, we suppose the windage to be no more, than is just necessary for the easy putting down the bullet; whereas, in real service, either through negligence or unskilfulness, it often happens, that the diameter of the bore so

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much exceeds the diameter of the bullet, that great part of the inflamed fluid escapes by its side; whence the velocity of the shot, in this case, may be considerably less than what we have assigned. However, part of this may possibly be compensated by the greater heat, which (as we have observed in the sixth proposition) in all probability attends the firing of these large quantities of powder.

COROLLARY.

From the great velocity of cannon-shot, assigned in this proposition we may clear up that difficulty, which has driven some writers, on the common theory of gunnery, into a very extraordinary hypothesis. The difficulty, I mean, is the extent of the supposed point-blank shot, or the distance to which it is conceived to fly in a straight line. Our *Anderson* having found, by many experiments, that the track of shells and bullets, in the first part of their motion, was much less incurvated, than what it ought to be on the principles of *Galileo*, when compared with the distant ranges, he supposed, in order to reconcile this circumstance with his theory, that every shot was impelled to a certain distance from the mouth of the piece, in a straight line, or that for some distance it was no ways affected by the action of gravity. By this means he defended, as he thought, the hypothesis of a parabolic motion, and at the same time assented to the vulgar opinion of the practical writers, who, in general, asserted the same thing. But could no better account be given of his experiments, it would yet be unnecessary, I presume, formally to confute so strange a supposition as that of the suspension of the action of gravity. Indeed, *Anderson* was deceived, by his not knowing how greatly the primitive velocity of the

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the heaviest shot is diminished in the course of its flight by the resistance of the air. And the received opinion of practical gunners, is not more difficult to account for, since, when they agree, that every shot flies in a straight line to a certain distance from the piece, which imaginary distance they have denominated the extent of the point-blank shot, we need only suppose, that within that distance, which they thus determine, the deviation of the path of the shot from a straight line is not very perceptible in their method of pointing. Now, as a shot of 24lb. fired with two thirds of its weight in powder, will, at the distance of 500 yards from the piece, be separated from the line of its original direction, by an angle of little more than half a degree; those, who are acquainted with the inaccurate methods often used in the directing of cannon, will easily allow, that so small an aberration as this may, by the generality of practitioners, be unattended to, and the path of the shot may consequently be deemed a straight line, especially as other causes of error will often intervene, much greater than what arises from the incurvation of this line by gravity.

In the present proposition, the velocity of a shot is determined, both when fired with two thirds of its weight of powder, and with half its weight of powder, respectively; and, on this occasion, I must remark, that on the principles of the theory, which we have ascertained in this treatise, the increasing the charge of powder will increase the velocity of the shot, till the powder arrives at a certain quantity; after which, if the powder be increased, the velocity of the shot will diminish. The quantity producing the greatest velocity, and the proportion between that greatest velocity and the velocity communicated by greater and lesser charges, may be thus assigned.

Let

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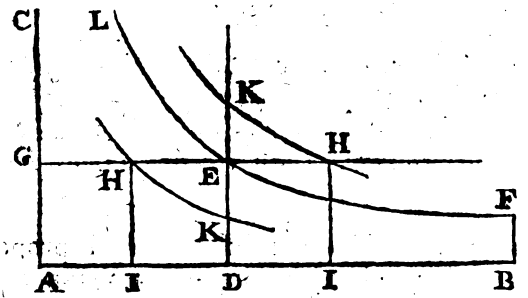

Let AB represent the axis of the piece; draw AC perpendicular to it, and to the asymptotes AC and AB, describe any hyperbola LF, and draw BF parallel to AC; find out now the point D, where the rectangle ADEG is equal to the hyperbolic area DEFB, then will AD represent that heighth of the charge, which communicates the greatest velocity to the shot; whence AD being to AB, as 1 to 2,71828, as appears by the table of logarithms, from the length of the line AD, thus determined, and the diameter of the bore, the quantity of powder, contained in this charge, is easily known*.

If, instead of this charge, any other, filling the cylinder to the height AI, be used, draw IH parallel to AC, and through the point H, to the same asymptotes AC and AB, describe the hyperbola HK; then the greatest velocity will be to the velocity communicated by this charge AI, in the subduplicate proportion of the rectangle AE, to the same rectangle diminished by the trilinear space HKE. All this easily follows from the principles laid down in the 7th proposition of the 1st chapter.

PROP.

* See this rule otherwise investigated in my Course of Mathematics, vol. 2. p. 351. edit. 4th. But the same thing determined by experiments is rather less than this number 2,71828, in all the various lengths of cannon; as may be there seen. H.

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PROP. V.

When a Cannon-Ball of 24lb. weight, fired with a full Charge of Powder, first issues from the Piece, the Resistance of the Air on its Surface amounts to more than twenty Times its Gravity.

For we have shewn, in the second proposition of the present chapter, that the resistance of the air on the surface of a bullet of $\frac{1}{4}$ of an inch diameter, moving with a velocity of 1670 feet in 1″, amounted to 10lb. Now we have seen, in the last proposition, that an iron bullet weighing 24lb. if fired with 16lb. of powder, (which is usually esteemed its proper battering charge) acquires a velocity of about 1650 feet in 1″, scarcely differing from the other; whence, as the surface of this last bullet is more than fifty-four times greater than the surface of a bullet of $\frac{1}{4}$ of an inch in diameter, and their velocities are nearly the same, it follows that the resistance on the larger bullet will amount to more than 540lb.* which is near twenty-three times its own weight.

SCHOLIUM.

We have observed, in the introduction, that the theorists, who have professedly written on the subject of gunnery, have generally agreed in supposing the flight of shot and shells to be nearly in the curve of a parabola; and it is against this hypothesis that the two last propositions are particularly aimed.

For the reason, which has been given by these authors, in support of their opinion, is the supposed

* By my experiments, the resistance amounts to more than 560lb. or 24 times the weight of the ball. H.

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posed inconsiderable resistance of the air; since, as it is agreed on all sides, that the track of projectiles would be a parabola, if there was no resistance; it has from thence been too rashly concluded, that the interruption, which the ponderous bodies of shells and bullets would receive from so rare a medium as the air, would be scarcely sensible, and consequently that their parabolic flight would be hereby scarcely affected.

Now the prodigious resistance of the air to a bullet of 24lb. weight, such as we have here established it, sufficiently confutes this reasoning; for how erroneous must that hypothesis be, which neglects as inconsiderable, a force, which amounts to more than twenty times the gravity of the moving body? However, we shall not content ourselves with having demonstrated the reality and quantity of the air's resistance, but we shall proceed to a more particular examination of the flight of bodies in that medium, where we shall evince, by many experiments, how greatly the track, described by almost every projectile, deviates in every circumstance from what it ought to be on the generally-received principles. But, first, it is necessary to assume a few particulars, the demonstrations of which may be found in almost every writer on the common theory of falling bodies.

Post. 1. If the resistance of the air be so small, that the motion of a projected body be in the curve of a parabola; then the axis of that parabola will be perpendicular to the horizon, and consequently the part of the curve, in which the body ascends, will be equal and similar to that in which it descends.

Post. 2. If the parabola, in which the body moves, be terminated on a horizontal plain; then the vertex of the parabola will be equally distant from its two extremities.

Post. 3. Also the moving body will fall on that horizontal

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horizontal plain in the same angle and the same velocity, with which it was first projected.

Post. 4. If a body be projected in different angles, but with the same velocity; then its greatest horizontal range will be, when it is projected in an angle of 45° with the horizon.

Post. 5. If the velocity, with which the body is projected, be known, then this greatest horizontal range may be thus found: compute, according to the common theory of gravity, what space the projected body ought to fall through to acquire the velocity, with which it is projected; then twice that space will be the greatest horizontal range, or the horizontal range, when the body is projected in an angle of 45° with the horizon.

Post. 6. The horizontal ranges of a body, when projected with the same velocity, at different angles, will be between themselves, as the sines of twice the angle, in which the line of projection is inclined to the horizon.

Post. 7. If a body be projected in the same angle with the horizon, but with different velocities; the horizontal ranges will be in the duplicate proportion of those velocities.

These postulates contain the principles, on which the motions of projectiles are computed by the modern writers on the art of gunnery.

If any of these postulates hold not true, when applied to the motion of a projectile, then that projectile deviates in its flight from a parabolic track; we shall therefore effectually destroy the common theory of projectiles, if we can shew, that, in general, none of these postulates correspond to the observed motions of those bodies.

PROP.

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PROP. VI.

The Track described by the Flight of Shot or Shells is neither a Parabola, nor nearly a Parabola, unless they are projected with small Velocities.

FOR we have determined, in the fourth proposition of the present chapter, that a musket-ball $\frac{3}{4}$ of an inch in diameter, fired with half its weight of powder, from a piece 45 inches long, moves with a velocity of near 1700 feet in 1″. Now, if this ball flew in the curve of a parabola, its horizontal range at 45° would be found, by the fifth postulate, to be about 17 miles. Now all the practical writers assure us, that this range is really short of half a mile. *Diego Ufano* assigns to an arquebuse, 4 feet in length, and carrying a leaden ball of $1\frac{1}{2}$ oz. weight (which is very near our dimensions) an horizontal range of 797 common paces, when it is elevated between 40 and 50 degrees, and charged with a quantity of fine powder equal to the weight of the ball. *Marsenus* too tells us, that he found the horizontal range of an arquebuse at 45° to be less than 400 fathom, or 800 yards; whence, as either of these ranges are short of half an *English* mile, it follows that a musket-shot, when fired with a reasonable charge of powder, at an elevation of 45°, flies not the $\frac{1}{3\,4}$ part of the distance it ought to do, if it moved in a parabola.

Nor is this great contraction of the horizontal range to be wondered at, when it is considered, that the resistance of this bullet, when it first issues from the piece, amounts to 120 times its gravity; as has been experimentally demonstrated in the second proposition of the present chapter.

Again, lest it should be said, that this aberration of the flight of a musket-ball from the curve

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of a parabola, is no proof but that heavier shot, whose resistance is much less in proportion to their weight, may sufficiently coincide with the common hypothesis; our next instance shall be in an iron bullet of 24lb. weight, which is the heaviest in common use for land service. Such a bullet, fired from a piece of the customary dimensions, with its greatest allotment of powder, has a velocity of 1650 feet in 1″, as we have determined in the fourth proposition of the present chapter.—Now if the horizontal range of this shot, at 45° be computed on the parabolic hypothesis by the fifth postulate, it will come out to be about 16 miles, which is between five and six times its real quantity; for the practical writers all agree in making it less than three miles: and St. *Remy* informs us of some experiments made by Mr. *du Metz*, in which the range, at 45°, of a piece ten feet in length, carrying a ball of 24lb. and charged with 16lb. of powder, was 2250 *French* fathom, which is 222 fathom short of three miles; consequently an iron bullet of 24lb. weight, when impelled with its full allotment of powder, flies not, at 45°, to the fifth part of the distance which it ought to do, if it described the curve of a parabola.

But farther, it is not only when projectiles are moved with these very great velocities, that their flight sensibly varies from the curve of a parabola; the same aberration often takes place in such, as move slow enough to have their motion traced out by the eye; for there are few projectiles, that can be thus examined, which do not visibly disagree with the first, second, and third postulate, they obviously descending through a curve which is shorter, and less inclined to the horizon, than that in which they ascended; also the highest point of their flight, or the vertex of the curve, is much nearer to the place, where they fall on the ground, than

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than to that from whence they were at first discharged. These things cannot be a moment doubted of by one, who in a proper situation views the flight of stones, arrows, or shells, thrown to any considerable distance.

I have found too by experience, that the fifth, sixth and seventh postulates are excessively erroneous, when applied to the motions of bullets moving with small velocities: a leaden bullet $\frac{1}{4}$ of an inch in diameter, discharged with a velocity of about 400 feet in 1″, and in an angle of 19° 5′, with the horizon, ranged on the horizontal plane, no more than 448 yards; whereas its greatest horizontal range being found by the fifth postulate, to be at least 1700 yards, the range at 19° 5′, ought, by the sixth postulate, to have been 1050 yards; whence, in this experiment, the range was not $\frac{1}{7}$ of what it must have been, had the common received theory been true.

Again, a ball was fired with the same velocity as in the last experiment, but at an elevation of 9° 45′, its horizontal range was at a medium 330 yards.

Now this range, according to the fifth and sixth postulates, (if its original velocity be considered) should have been 566 yards. But if it were to be deduced from the last experiment, by means of the sixth postulate, it should have been no more than 241 yards; either of which numbers are extremely distant from 330.

Again, a ball being fired at an elevation of 8°, but with a velocity of 700 feet in 1″, the horizontal range at a medium was 690 yards.

But computing this range from the original velocity of the projected body, according to the fifth and sixth postulates, we shall find, that if the theory, on which those postulates are founded, could be relied on, the range in the present instance ought to have been 1400 yards; whence it appears, that

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the body flew not to half the distance which, had it moved in a parabola, it ought to have done.

Again, a ball being fired with the same velocity as in the last, but at an elevation of 4°, its horizontal range was 600 yards.

Now this range, if deduced from the last experiment by the sixth postulate, should not have been more than 350 yards; hence then is evinced the falsity of that postulate, and consequently of the parabolic hypothesis, on which it is founded.

Having thus proved, that the track described by the flight, even of the heaviest shot, is neither a parabola, nor approaching to a parabola, except when they are projected with very small velocities; we shall refer to a second part, a more distinct explication of the nature of the curve, which these bodies really trace out in their motion through the air: but, as a specimen of the great complication of that subject, I shall here insert an account of a very extraordinary circumstance, which frequently takes place therein.

As gravity acts perpendicularly to the horizon, it is evident, that if no other power but gravity deflected a projected body from its rectilinear course, its motion would be constantly performed in a plane perpendicular to the horizon, passing through the line of its original direction: but we have found, that the body in its motion often deviates from this plane, sometimes to the right-hand, and at other times to the left; and this in an incurvated line, which is convex towards that plane; so that the motion of a bullet is frequently in a line having a double curvature, it being bent towards the horizon by the force of gravity, and again bent out of its original direction, to the right or left, by the action of some other force: in this case no part of the motion of the bullet is performed in the same plane, but its track will lie in the surface of a kind of cylinder, whose axis is

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perpendicular to the horizon. The truth of this assertion we shall evince by indisputable experiments.

PROP. VII.

Bullets in their Flight are not only depressed beneath their original Direction by the Action of Gravity, but are also frequently driven to the right or left of that Direction by the Action of some other Force.

If it was true, that bullets varied their direction by the action of gravity only, then it ought to happen, that the errors, in their flight to the right or left of the mark they were aimed at, should increase in the proportion of the distance of the mark from the piece only: but this is contrary to all experience; the same piece, which will carry its bullet within an inch of the intended mark, at 10 yards distance, cannot be relied on to 10 inches in 100 yards, much less to 30 inches in 300 yards. This increase of the uncertainty of the shot in great distances, more than in the proportion of those distances, must have been observed by all, who have been at any time conversant with the practical part of artillery. Now this inequality can only arise from the track of the bullet being incurvated sideways as well as downwards; for by this means the distance between that incurvated line, and the line of direction, will increase in a much greater ratio than that of the distance; these lines being coincident at the mouth of the piece, and afterwards separating in the manner of a curve and its tangent, if the mouth of the piece be considered as the point of contact.

But that those, who have not been themselves accustomed to those matters, may entertain no doubt about what we here assert; I shall recite

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some experiments, I have made, which will put the matter out of all question.

I took a barrel carrying a ball of $\frac{3}{4}$ of an inch diameter, and fixing it on a heavy carriage, I satisfied myself of the steadiness and truth of its direction, by firing at a board $1\frac{1}{7}$ foot square, which was placed at 180 feet distance; for I found, that in 16 successive shot I missed the board but once. Now the same barrel being fixed on the same carriage, and fired with a smaller quantity of powder, so that the shock on the discharge would be much less, and consequently the direction less changed, I found, that at 760 yards distance, the ball flew sometimes 100 yards to the right of the line it was pointing on, and at other times 100 yards to the left. I found too, that its direction in the perpendicular line was not less uncertain, it falling one time above 200 yards short of what it did at another; although, by the nicest examination of the piece after the discharge, it appeared not to have the least started from the position it was placed in.*

This then sufficiently confirms the proposition, since it was impossible the bullet could have flown in the manner here described, had not the line of its flight been bent round to the right or left as well as downwards.

SCHOLIUM.

The reality of this doubly-incurvated track being thus demonstrated, it may perhaps be asked, what can be the cause of a motion so different from

* The uncertainty of shooting with these leaden bullets appears to be very great: it is not probable, however, that it can be so great in shooting with cannon-balls; because these are commonly cast rounder, and, being of iron, have not their figure changed by the action of the powder, nor by striking against the sides of the bore. H.

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from what has been hitherto supposed? And to this I answer, that the deflection in question must be owing to some power acting obliquely to the progressive motion of the body, which power can be no other than the resistance of the air.* If it be farther asked, how the action of the resistance of the air can at any time be in a line oblique to the progressive motion of the body? I farther reply, that it may sometimes arise perhaps from inequalities in the resisted surface, but that its general cause is doubtless a whirling motion acquired by the bullet about its axis; for by this motion of rotation, combined with the progressive motion, each part of the bullet's surface will strike the air in a direction very different from what it would do, if there was no such whirl; and the obliquity of the action of the air arising from this cause will be greater, according as the rotatory motion of the bullet is greater in proportion to its progressive motion.

I have now finished all that I proposed to determine in this place, relating to the force of powder and the resistance of the air: but as the knowledge of the resistance of solids to the penetration of shot is of great importance in the practical part of gunnery, especially in battering in breach; I shall end the present treatise with a proposition relating thereto, which is as follows.

* This assertion would require some modification, as the chief part of the deviation in the flight must be owing to the irregular formation of the ball, both as to its internal structure, and want of roundness on the surface. H.

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PROP. VIII.

If Bullets of the same Diameter and Density impinge on the same solid Substance with different Velocities, they will penetrate that Substance to different Depths, which will be in the duplicate Ratio of those Velocities nearly. And the Resistance of solid Substances to the Penetration of Bullets is uniform.

THE first part of this proposition I have found to be true in a great number of instances; for when a leaden bullet $\frac{3}{4}$ of an inch in diameter, was fired against a solid block of elm, with a velocity of about 1700 feet in 1″, I found, that in a great number of trials it had penetrated from $4\frac{1}{2}$ to $5\frac{1}{2}$ inches deep. When a bullet of the same size was fired against the same block, with a velocity of about 730 feet in 1″, its outer surface was always near $\frac{1}{4}$ of an inch within the surface of the wood, so that its penetration was at a medium about 1 inch; or, if the cavity be considered, and reduced to a cylinder, about $\frac{7}{8}$ of an inch; and with a velocity of 400 feet in 1″, the bullet penetrated the same block usually to about half its substance, which, reduced to a cylindric cavity is $\frac{1}{4}$ of an inch in depth.

Now 55, 10, 3, are nearly in the duplicate proportion of these velocities; whence, if the penetration to the greatest velocity be supposed 5 inches, the penetration of the others ought by the proposition to be $\frac{10}{11}$ and $\frac{3}{11}$ of an inch respectively; and these numbers scarcely differ from $\frac{7}{8}$ and $\frac{1}{4}$, which are what we have found in our experiments; a greater coincidence than this cannot be expected, when the unequal texture of the same piece of wood, and the change of the form of the bullet by the stroke, are considered.

Now, from the penetration being in the duplicate

cate proportion of the velocity of the impinging body, the uniform resistance of the wood is easily evinced on the same principles, that the uniform action of gravity is demonstrated from its communicating to falling bodies velocities in the subduplicate proportion of the spaces they descend through, or from the rising of bodies when projected upwards, to heights which are in the duplicate proportion of the velocities, with which they begin to ascend.*

* These experiments with leaden bullets, which change their figure by the stroke, are not so proper to determine this point, as cast iron balls, with which I have made many experiments; by which it appears both that the penetrations, with the higher charges, fall short of the above proportion; and that the resisting force of the wood is not a constant quantity. See some of these experiments in several places of my Tracts, and some remarks in particular at p. 265. H.

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SUBSEQUENT TRACTS.

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An Account of a Book entitled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder; and an Investigation of the Resisting Power of the Air to swift and slow Motions; as far as the same relates to the Force of Gunpowder. Read before the Royal Society, April 14 and 21, 1743, and printed in the Philosophical Transactions, No. 469.

THIS treatise contains two chapters. The first treats of the force of gunpowder, and the velocities communicated to bullets by its explosion: the second considers the resistance of the air to bullets and shells moving with great velocities; and endeavours to evince, that this resistance is much beyond what it is generally esteemed to be; and consequently, that the track described by the flight of these projectiles, is very different from what is usually supposed by the modern writers on this subject.

The principal points endeavoured to be established in the first chapter are these, "That the force "of fired gunpowder is no more than the action "of a permanent elastic fluid, which is produced "by the explosion; that this fluid observes the "same laws with common air in their exertion of "its pressure or elasticity;" and consequently, "That the velocities communicated to bullets by "the explosion, may be easily computed from "the common rules, which are established for "the determination of the air's elasticity."

The two first propositions contain the proofs, that a permanent elastic fluid is constantly generated in the explosion of gunpowder; this is evinced

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evinced by well-known experiments daily repeated, and acquiesced in by all who have frequented the usual courses of experimental philosophy, of which these experiments generally make a part; so that the author presumes he may consider this point as incontestibly established, at least he has never yet met with any who have questioned it.

The third proposition is, that the elasticity of this fluid produced by the firing of gunpowder, is, *cæteris paribus*, directly as its density; and the experiment by which this was confirmed, was letting fall separately two quantities of powder, the one double the other, on a red-hot iron included in an exhausted receiver; and it appeared, by the descent of the mercury, that the elasticity of the fluid produced from the double quantity of powder, was nearly double the elasticity of that produced from the single quantity; that is, the elasticity was nearly as the density of the fluid.

But it may perhaps be thought, that a single experiment is too slender a foundation, on which to build so material a principle; since all subsequent reasonings on the force of powder in some measure depend on it. In reply to this it may be said, that the author recited this single experiment on account of the great quantity of powder made use of in it, which was three sixteenths of an ounce; but that he had really made many more equally conclusive, which he thought it unnecessary to mention. However, those who doubt of this proposition, may satisfy themselves herein by some experiments made by the late Mr. *Hauksbee* before this SOCIETY, though with a different view; where, by the firing of twenty-six quantities of powder successively, the mercurial gage was sunk from twenty-nine inches and a half, to twelve three fourths; for by comparing these experiments together, and making the necessary allowances, it will be found, that the elasticity

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ticity was nearly proportional to the density in all that variety of densities.

In this proposition, the analogy between the fluid produced by the explosion of powder and common air, is established thus far, that they exert equal elasticities in like circumstances; for this variation of the elasticity, in proportion to the density, is a well-known property of common air. But other authors, who, since the time of Mr. *Boyle*, have examined the factitious elastic fluids produced by burning, distillation, &c. have carried this analogy much farther, and have supposed these fluids to be real air, endued with all the properties of that we breathe; particularly the reverend Dr. *Hales*, who has pursued this examination with the greatest exactness, in a series of the best contrived processes, constantly affixes the denomination of air to these factitious fluids; he having found, that their weight is the same with that of common air, and that they dilate with heat, and contract with cold; and that they vary their densities under different degrees of impression, in the same proportion with common air; and from hence, and other circumstances of agreement between them, he supposes them to be of the same nature with air, and conceives them to be fitly designed by the same name.

But so perfect a congruity between these factitious fluids and air is not necessary for the purposes of this treatise. The fundamental positions of this first chapter supposing no more, than that the elasticity of the fluid, produced in the explosion of gunpowder, is always, *cæteris paribus*, as its density; and that the force of fired gunpowder is only the action of that fluid modified according to this law. It has been already mentioned, on what grounds the first of these principles hath been asserted, as contained in the third proposition; and it remains to explain the reasons urged

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urged for the support of the last in the eight succeeding propositions.

The law of the action of this fluid being determined, two methods offer themselves for investigating the absolute force of powder on the bodies it impels before it. The first by examining the quantity of this fluid produced by a given quantity of powder, and thence finding its elasticity at the instant of the explosion; the other by determining the actual velocities communicated to bullets by known charges, acting through barrels of different dimensions. The first is the most easy and obvious, but the second the most accurate method; and therefore the author has separately pursued each, and he has found that their concurrence has greatly exceeded his expectation, and thereby both of them receive an additional confirmation.

The quantity of the elastic fluid, produced by the firing of a given quantity of powder, is determined by firing it in an exhausted receiver, and observing how much the mercurial gage subsides thereby, making a proper allowance for the increase of its elasticity from the heat of the included hot iron. But then, as the subsiding of the mercury is not measured till the flame of the powder is extinguished, and the flame is reduced somewhat near the temperature of the external air; it is evident that the elasticity thus estimated is much short of what it really was in the instant of explosion; and, therefore, to obtain that elasticity, which is the force sought, it is necessary to make some estimate of the increase of the elasticity of the fluid by the fire and flame of the explosion. For this purpose it is examined in the fifth proposition, how much the elasticity of common air is increased by a degree of heat equal to that of iron beginning to grow white hot; and it is found, at a medium, to be thereby augmented something

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something more than four times; whence, as the fluid produced by any quantity of gunpowder takes up, when compressed by the weight of the incumbent atmosphere, a space something less than 250 times the bulk of the powder; it follows, that if its elasticity, in the instant of explosion, be supposed to be increased in the same proportion with that of the air last mentioned, it becomes by this means about 1000 times greater than the pressure of the atmosphere; that is, conceiving it to be contained in that space only which the powder occupied, before it was fired.

Those who have not been conversant in these experiments, may possibly suppose that the elasticity of the powder, at the instant of explosion, may be immediately known by the first sudden descent of the mercury: but many circumstances concur to render this method impracticable; amongst the rest, it must be remembered, that some air is constantly left in the receiver, which is heated by the blast, and unites its effects, in the first instant, with the action of the powder; besides, the first descent may be varied, by varying the tube, although all things else remain unchanged.

By the method hitherto described, it is collected, that the elasticity of the fluid produced from fired gunpowder, when contained in the space, which was taken up by the powder before the explosion, is about 1000 times greater than the elasticity of common air, or, which is the same thing, 1000 times greater than the pressure of the atmosphere.

But, besides the determination of the quantity of fluid, produced by a given quantity of powder, (the method on which this deduction is founded) there is another method of discovering the same thing, which, though less obvious, is yet (as hath been already observed) more accurate: that is,

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by examining the actual velocities communicated to bullets by the explosion of given charges in given cylinders; and this is the subject of the 7th, 8th, and 9th propositions.

And first, it is evident, that this examination cannot take place, unless a method of discovering the velocities of bullets be previously established. Now the only known means of effecting this was, either by observing the time of the flight of bullets through a given space; or by finding their ranges when they were projected at a given angle, and thence computing their velocity on the hypothesis of their parabolic motion. The first of these methods was often impracticable, and in all great velocities extremely inaccurate, both on account of the shortness of the time of their flight, and the resistance of the air. The second is still more exceptionable; since, by reason of the air's resistance, the velocities thus found may be less in any *ratio* given, than the real velocity sought. Now, to avoid these difficulties, the author has invented a method of determining the velocities of bullets, which may be carried to any required degree of exactness, and is no ways liable to the foremen-tioned exceptions; for, by this invention, the velocity of the bullet is found in any point of its track, independent of the velocity it had before it arrived at that point, or of the velocity it would have, after it had passed it: so that not only the the original velocity, with which it issues from the piece, is hence known, but also its velocity after it has passed to any given distance; and therefore the variations of its velocity from the resistance of the air may be also ascertained with great facility. The machine for this purpose is described in the 8th proposition, and the principle it is founded on, is this simple axiom of mechanics; *That if a body in motion strikes on another at rest, and they are not separated after the stroke, but move on with one*

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one common motion; then that common motion is equal to the motion, with which the first body moved before the stroke: whence if that common motion and the masses of the two bodies are known, the motion of the first body before the stroke is thence determined. On this principle then it follows, that the velocity of a bullet may be diminished in any given *ratio*, by its being made to impinge on a body of a weight properly proportioned to it; and hereby the most violent motions, which would otherwise escape our examination, are easily determined by these retarded motions, which have a given relation to them. Hence then, if a heavy body greatly exceeding the weight of the bullet, whose velocity is wanted, be suspended, so that it may vibrate freely on an axis in the manner of a pendulum, and the bullet impinges on it, when it is at rest; the velocity of the pendulum after the stroke will be easily known by the extent of its vibration; and from thence, and the known relation of the weight of the bullet and the pendulum, and the position of the axis of oscillation, the velocity, with which the bullet is impinged, will be determined, as is largely explained in the 8th proposition. Where note, that there is a paragraph by mistake omitted in that proposition, which should increase the velocity there found in the subduplicate proportion of the distances of the points of oscillation and percussion from the axis of suspension; but this only affects that particular number, for it was remembered in the computations of the succeeding experiments, the numbers of which are truly stated.

It being explained how the velocities of bullets may be discovered by experiment: the next consideration is, from those velocities to determine the force, which produced them.

And the author thought, the best method of effecting this was by computing, what velocities

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would arise from the action of fired powder, supposing its force to be rightly assumed by the process in the preceding part; that is, supposing the elasticity of the fluid thence arising to be at first 1000 times greater than that of common air; for then, by comparing the result of these computations with a great number of different experiments, it would appear, whether that force was rightly assigned; and if not, in what degree it was to be corrected.

Preparatory to this computation, the author assumes, in his 7th proposition, these two principles:

1*st*, That the action of the powder on the bullet ceases, as soon as the bullet is got out of the piece.

2*dly*, That all the powder of the charge is fired, and converted into an elastic fluid, before the bullet is sensibly moved from its place.

And in the annexed scholium he has given the arguments and experiments, which induced him to rely on these postulates; all which is necessary at present to discuss more at large.

If the force of gunpowder was supposed capable of being determined with the same accuracy and rigour, which takes place in subjects purely geometrical, the first of these postulates would be doubtless erroneous, since it cannot be questioned but the flame acts in some degree on the bullet after it is out of the piece.

But it is well known, that, in experimental subjects, no such preciseness is attainable; for those versed in experiments perpetually find, that either the unavoidable irregularities of their materials, or the variation of some unobserved circumstance, occasion very discernible differences in the event of similar trials. Thus the experiments made use of for confirming the laws of the collision of bodies, have never been found absolutely to coincide either

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either with the theory, or with each other. The same is true of the experiments on the running and spouting of water and other fluids, and of the experiments made by Sir *Isaac Newton*, for the confirmation of his theory of resistances; in which, though they often differ from each other, and from that theory by one-twentieth, one-tenth, and even sometimes one-fifth part; yet those small inequalities have never been urged as invalidating his conclusions; since, in experiments of that nature, it was rather to be wondered at, that the difference between the different trials was so small.

And if some minute irregularities are the necessary concomitants of all complicated experiments, it may be well supposed, that the action of so furious a power as that of fired gunpowder, which visibly agitates and disorders all parts of the apparatus made use of, cannot but be attended with sensible variations; and it in fact appears, that in the table of experiments inserted in the 9th proposition, the velocities of bullets fired from the same piece, charged with the same powder, and all circumstances as near as possible the same, do yet differ from each other by one-fiftieth, one-fortieth, and sometimes more than one-thirtieth of the whole; and yet the author does not conceive, that these small differences are any exception to the conclusiveness of his principles; but, he presumes, that had he pretended, without disclosing his method, to have computed the force of powder, and the velocities of bullets, in different circumstances, to a much less degree of accuracy than this, he should have been censured, as boasting of what would have been thought impracticable.

If then the action of the flame on the bullet, after it is out of the piece, is so small as to produce no greater an effect, than what may be destroyed by the inevitable variations of the experiments, the neglecting it entirely, and supposing no such

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force to take place, is both a convenient and a reasonable procedure: for, indeed, without the assumption of postulates of this kind, it were impossible to have proceeded one step in natural philosophy; since no mechanic problem hath been ever solved, in which every real inequality of the moving force hath been considered.

Now what induced the author to suppose, that this postulate (though not rigorously true) might be safely assumed, was the consideration of the spreading of the flame by its own elasticity, as soon as it escapes from the mouth of the piece: for by this means he conceived, that the part of it, which impinged on the bullet, might be safely neglected; although the impulse of the entire flame was a very remarkable force.

With regard to the second postulate, "That all "the powder is fired before the bullet is sensibly "moved from its place;" it is incumbent on the author to be still more explicit, as this SOCIETY did some time since appoint a committee for examining this very position, who, after making a great number of experiments, have determined*, *That all the powder is not fired, before the bullet is sensibly moved from its place;* and they have, at the same time, assigned the quantities remaining unfired under different circumstances.

These determinations of the committee are most true; but the author must observe, that from the experiments recited by them, and the quantity of unfired powder, which they collected, it may be concluded, that in a barrel of a customary length, charged with the usual quantity of powder, the deficiency of velocity occasioned by the powder remaining unfired will be scarcely sensible; and in the shortest barrel ever used by the author, where the space, the bullet was impelled through, was not five inches, and where of course this deficiency of

* See these *Transactions*, No. 465. p. 172, &c.

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of velocity ought to be the greatest, it cannot amount to one-thirtieth part of the whole; and consequently this postulate, though not rigorously true, may yet be safely assumed, in the investigating the effects of powder. But before this is more particularly examined, it is necessary to explain the opinions, which have formerly taken place on this subject.

Those who have hitherto wrote on the manner, in which powder takes fire, have supposed it to be done by regular degrees; the first grains firing those contiguous, and they the next successively; and it has been generally thought, that a considerable time was employed in these various communications: for Mr. *Daniel Bernoulli*, in his excellent *Hydrodynamica*, has concluded from some experiments made at *Petersburgh*, that the greatest part of the charge escapes out of the piece unfired, and that the small part, which is fired, does not take fire till it is near the mouth. Many theories too have been composed on the time of the progress of the fire amongst the grains, and the different modifications, which the force of powder did thence receive; and it has been generally conceived, that the proper lengths of pieces were determinable from this principle: "That "they should be long enough to give time for all "the powder to fire."

But the author being satisfied, that no such regular and progressive steps could be observed in the explosion; and having found, that by loading with a greater weight of bullet, and thereby almost doubling the time of the continuance of the powder in the barrel, its force received but an inconsiderable augmentation; and finding too, that doubling or trebling the usual charge, the powder thus added always produced a correspondent effect in the velocity of the bullet; and discovering likewise in a piece near four feet in

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length, charged with a usual charge of powder, that the velocity communicated to the bullet, during the first three inches of its motion, was full half the velocity, which it acquired in its whole passage through the barrel, and that the elasticity or force of the powder, in the first three inches of its expansion, was, at a medium, near eight times greater than in the last two feet of the barrel; he concluded from all these circumstances, that the time employed by the powder in taking fire was not necessary to be attended to in these computations; but that the whole mass might be supposed to be kindled, before the bullet was sensibly moved from its place.

And the experiments reported by the committee are the strongest proofs, (as far as they extend) that powder is not fired in the progressive manner usually supposed; for when the short barrel was charged with 12dw. and with 6dw. respectively, the quantity of powder which was collected unfired from 12dw. did not exceed by 3 grains, at a medium, what was collected from 6dw. although the bullet was a less time in passing through the barrel with 12dw. than with 6dw. it having a less way to move; consequently the quantity remaining unfired of the 6dw. did not continue unfired for want of time, since, when the piece was charged with 12dw. the additional 6dw. was consumed in a shorter time.

And again, when the barrel was so shortened, that the bullet, being placed close to the wad, lay with its outer surface nearly level with the mouth of the piece, so that it had not more than half an inch to move before the flame would have liberty to expand itself; yet, even in this short transit of the bullet, only 2dw. $1\frac{1}{2}$gr. was collected unfired, at a medium; which is about $\frac{1}{6}$ of the whole charge, or, if properly reduced, not more than $\frac{1}{12}$ of the charge: an obvious confutation of

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of the gradual firing of the powder in its passage through the barrel, and an easy proof, how small an error will be occasioned by supposing the whole charge to fire instantaneously; since the error in the velocity of the bullet, arising from a deficiency of $\frac{1}{12}$ of the charge, is $\frac{1}{24}$ of that velocity only.

I say, that the $\frac{1}{6}$ of the charge, which remained unfired, amounts to no more than $\frac{1}{12}$, when it is reduced as it ought. This reduction is founded on the other experiments reported by the committee, and on the circumstances of those trials, on which the author founded the present postulate. The author has supposed the powder, on which he reasons in this treatise, to be of the same sort with that made for the service of the government, a parcel of which he was favoured with by Mr. *Walton*. But this he chiefly kept for a standard, and generally used other powders, which, on examination, he found to be of equal force. These powders were of a very small and even grain, and the committee have found, that by sifting the government powder, and making use of the smaller grains, the quantity remaining unfired was less, at a medium, in the ratio of 5 to 3, than when it was used without sifting.

And again, it was found by extracting the saltpetre from the powder collected unfired, that there was less saltpetre contained in it than in real powder, and this nearly in the ratio of 9 to 7: these two proportions compounded, make the proportion of 15 to 7, and in this proportion must the quantities of powder collected unfired be reduced, in order to determine the quantities of real powder remaining unfired, in similar experiments made by the author.

And from hence it follows, that in the experiments made with a barrel $5\frac{1}{7}$ inches in length, where the ball had not 3 inches to move, and where the irregularity arising from the powder unfired ought to have been the most sensible; the quantity

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tity of real powder collected unfired from a charge of 12 dwt. would have been no more than 16 grains at a medium, or $\frac{1}{11}$ of the whole charge: and it being found by experiment, that the velocities of bullets placed in the same situation vary in the subduplicate proportion of the charges, the deficiency of velocity arising from the loss of the $\frac{1}{11}$ of the charge, would be about $\frac{1}{36}$ of the whole velocity only, which, in the present case, is not $\frac{2}{10}$ of an inch in the chord of the arch described by the pendulum measuring the velocity, and is a less difference, than what frequently occurs in the exactest repetition of the same experiments.

Other circumstances occur, which reduce the inequality arising from the unfired powder still lower; but it is thought, that this is fully sufficient to justify the postulate in question, especially as, in all cases of real use, the length of the barrel, in proportion to the quantity of the charge, will be much greater than in the present instance: whence the author presumes, that, in computing the velocities communicated to bullets by the action of powder, it may be safely supposed, that the whole charge is fired before the bullet is sensibly moved from its place; at least there is no foundation, from the experiments made on this subject by the committee, to suspect, that when small-grained powder is made use of, any greater irregularity will arise from the application of this supposition, than what would otherwise take place from the intervention of unavoidable accidents.

It has been thought necessary to discuss more at large these two postulates; because the last of them being almost in the very words of one of the questions proposed to be examined by the committee of this SOCIETY, and having by them been determined in the negative, those who have not attended to this subject might suppose, that thereby the author's principles were entirely overturned: now this would be a great injustice to him, since

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since he has not relied on this postulate as rigorously true; for he knew, and has himself taken notice in the present proposition, that some of the powder escapes unfired; and he has there made some conjectures on the cause of it: but, without insisting on the reality of those conjectures, he adds, that, "Be that as it may, the truth of our "position cannot in general be questioned."

And though it appears, from what has been already said, that the experiments recited by the committee rather confirm than invalidate the general sense of that postulate; yet it is but justice to own, that they are a full confutation of the conjectures of the author in relation to the cause, why some part of the powder comes out unfired; for the author has supposed, after *Diego Ufano*, that the part, which thus escaped, was scattered in the barrel, and not rammed up with the rest; or else that it was of a less inflammable composition: but the experiments made on this occasion entirely destroy this supposition.

As this, or any other conjecture on the cause of this accident, (for it plainly appears not to be for want of time only) has nothing to do with the general reasoning of the present treatise, it is not necessary to enter into it in this place; but it may not be improper to mention, that, on computing the quantities of powder collected from different charges, one of the committee was led to conjecture, that, what was thus collected, was only parts of grains, that had been fired; but were extinguished by the blast, before they were entirely consumed. This conjecture is strengthened by the extreme minuteness of the particles of all the powder, which was collected, and from the deficiency of the saltpetre found in it on examination: it may be added too, that the author, by gradually heating a parcel of powder, hath set it on fire, and blown it out again, for at least a dozen times successively;

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ly; and he will undertake to repeat the experiment at any time, if it should be doubted of.

The postulates, hitherto discussed, are preparatory to the 7th proposition. That proposition is employed in computing the velocity, which would be communicated to a bullet in a given piece by a given charge of powder, on the principles hitherto laid down; that is, supposing the elasticity of fired powder to be at first 1000 times greater than that of common air.

In the ninth proposition these computations are compared with a great number of experiments, made in barrels of various lengths, from seven inches to forty-five inches, and with different quantities of powder, from 6 dwt. to 36; and the coincidence between the theory and these experiments is very singular, and such as occurs in but few philosophical subjects of so complicated a nature.

By this agreement between the theory and the experiments, each part of the theory is separately confirmed; for by firing different quantities of powder in the same piece, and in the same cavity, it appears, that the velocities of the bullet, thence arising, are extremely near the subduplicate proportion of those quantities of powder, and this independent of the length of the piece: whence it is confirmed, that the elasticity of fired powder in various circumstances, is nearly as its density; and this does not only succeed in small quantities of powder, and in small pieces, but in the largest likewise, under proper restrictions; at least there are experiments, which could not be influenced by this theory, where the quantities of powder were above 100 times greater, than what are used by this author; and in these trials this circumstance takes place to sufficient exactness.

It is presumed then, that by this theory a near estimate may be always made of the velocities communicated to shells or bullets by given charges of powder;

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powder; at least these experiments evince, how truly the velocities of small bullets are hereby assigned; and the author can shew by the experiments of others, that in a shell of thirteen inches diameter, impelled by a full charge of powder, the same principle nearly holds: it is true indeed, that when the charge is much smaller than the usual allotment of powder, there are some irregularities, which are particularly mentioned at the end of the 9th proposition, to which head too, perhaps, must be referred the experiments made by the committee on the effect of different small chambers; but in the customary charges, the velocities of bullets resulting from all the experiments hitherto made, are really such, as the theory laid down in the preceding part of this treatise requires. And it appears, that these velocities are much greater, than what they have been hitherto accounted: and there are reasons from the theory to believe, that in cannon-shot the velocites may still exceed the present computation.

The ascertaining the force of powder, and thence the velocities of bullets impelled by its explosion, and the assigning a method of truly determining their actual velocities from experiments, are points, from whence every necessary principle in the formation or management of artillery may be easily deduced: considering, therefore, the infinite import of a well-ordered artillery to every state, the author flatters himself, that whatever judgment may be formed of his success in these inquiries, he will not be denied the merit of having employed his thoughts and industry on a subject, which, though of a most scientific nature, and of the greatest consequence to the public, hath been hitherto almost totally neglected; or, at least, so superficially considered, as to be left in a much more imperfect state than many other philosophical researches.

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With regard to the second chapter of this treatise, relating to the resistance of the air, the author has in his preface, mentioned his intention of annexing to it a series of experiments, on the real track of bullets, as modulated by that resistance: and therefore, as he proposes to complete those experiments this summer, unless unforeseen accidents prevent him; he chuses to postpone any account of the subject of the second chapter till that time; when he intends to lay the result of those experiments before this SOCIETY, in order that any exceptions or difficulties relating to them, may be examined and discussed, before they are published to the world.

*** *According to what Mr.* Robins *has said above, he composed the five following tracts, which are now first printed from his manuscript.*

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No. I.

OF THE

RESISTANCE OF THE AIR.

THAT all bodies moving through the air have their motions in some degree impeded by the resistance of that medium, is what, I presume, no person, however slightly acquainted with philosophical enquiries, will deny. But that the quantity of this retardation is so great, as to render it necessary to be considered in the determination of the ranges of all kinds of military projectiles, hath been often and vehemently denied. And this not so much by practitioners (whose opinions, though extremely inaccurate, have perhaps on this occasion been too much slighted) as by persons well skilled in mathematical learning, who have written professedly of the motions of projectiles, and whose treatises on this subject are generally esteemed and assented to. For, if it were necessary, a large catalogue of geometers of note might be here produced, who have asserted, in their works, that, in the operation of gunnery, the resistance of the air was too minute to merit attention. And I do not remember, that any author has formally or expressly contradicted this position; whatever may in general be concluded, from what some of them have at times advanced as to the laws observed by resisted bodies. Indeed (not to dwell longer on this point) I may venture to affirm, that it is now the almost universal opinion of those, who have studied the doctrine of projectiles from the treatises, which have hitherto been published thereon: "That all shells and bullets in their flight do near-

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" ly describe the curve of a parabola; and conse-
" quently, that the resistance of the air to the mo-
" tion of these bodies is altogether inconsiderable."

The prejudices, then, of many persons, whose reputations for knowledge weigh much with mankind, being thus contradictory to what I know to be fact, both from reiterated experiments of my own, and from geometrical deductions founded on the unquestioned experiments of others; it is highly necessary to remove, in the most unexceptionable manner, these false positions, authorized by great names, and confirmed by the prescription of near an age. For, till this is done, all attempts to compleat the theory and practice of gunnery must necessarily be ineffectual: since, as I am fully satisfied, that the resistance of the air is almost the only source of the numerous difficulties, which have hitherto embarrassed that science; it is evident, that till the reality and efficacy of this resistance is once established, it will be impossible to procure a candid hearing to any discoveries that may depend thereon, but, on the contrary, every speculation of this kind will be treated as ridiculous and chimerical.

To succeed then, in this necessary preliminary, I shall advance the two following propositions.

First, That the resistance of the air to a twelve pound iron bullet, moving with a velocity of 25 feet in a second, is not less than half an ounce avoirdupois.

Second, That this resistance is nearly in the duplicate proportion of the velocity of the resisted body; that is, that it is four times as much, when the resisted body moves with twice the velocity; nine times as much, when it moves with three times the velocity, and so on.

These propositions may be deduced from the experiments recited in the 8th section of the 2d book of Sir *Isaac Newton's* Principia. But to remove all

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all doubt about them, and to prevent all future exceptions; I do not choose to rely upon any authority whatever, but I propose to repeat to this society some experiments of my own, which will evince the truth of them by obvious and ocular demonstrations.

Indeed, with respect to the second of these propositions, it is true, that in great changes of velocity there is some variation from the rule here laid down; but this is a variation, that will on the whole increase, and not diminish the resistance.—And it is to be remembered, that I am not now nicely assigning the laws observed by this resistance in the most rigid and accurate sense; but am endeavouring, from a more lax and general idea of its efficacy, to evince the necessity of its being considered in the future theory of projectiles.

As I have often repeated the experiments, which I immediately intended to produce to the society, in proof of these propositions, I have no doubt of their success; I shall therefore for an instant suppose, that they have succeeded, and that the law of the air's resistance and its efforts against a 12lb. shot, moving with a velocity of 25 feet in a second, is thereby determined to be, what I have above assigned; and from hence it will follow, that to a velocity of 100 feet in a second (that is 4 times 25) the resistance of the air to the same shot is not less than 16 half ounces, or half a pound avoirdupois, and, proceeding on, we shall find, that to a velocity of 500 feet in a second, it is not less than 12lb.$\frac{1}{2}$; and to a velocity of 1000 feet in a second, not less than 50lb. and to a velocity of 1700 feet in a second, not less than 144lb.$\frac{1}{2}$: and this last velocity of 1700 feet in a second not being very distant from that, with which a twelve pound shot is discharged from a cannon, and the number 144lb.$\frac{1}{2}$ being very near 12 times 12; it follows, that the resistance of the air to a 12lb.

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shot, when fired with its usual charge of powder, is not less than 12 times its own weight. Now that the motion of this body can be rightly assigned from the consideration of its gravity and original velocity only, whilst a disturbing force, not less than 12 times its gravity, is utterly neglected, is a position so obviously absurd, that I flatter myself, when the experiments, which I am now going to repeat*, are attentively weighed, the reality and efficacy of the air's resistance will be no longer doubted, nor the necessity of considering it in almost all the operations of gunnery, any longer contested. And these points being once established, I propose, in some future papers, to recount the changes which will hence arise in the motions of shells and bullets; and to exhibit a method of computation adapted to the real circumstances of projectiles resisted by the air, which, though truly representing the actual motions of these bodies, is yet, in many instances, as easy and as concise as the erroneous computations founded on the parabolic hypothesis.

* The experiments exhibited upon this occasion are hereafter described in the first part of the third paper.

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No. II.

OF THE

RESISTANCE OF THE AIR;

Together with the Method of computing the Motions of Bodies projected in that Medium.

As I have not yet heard, that any objections have been made to the conclusiveness of the experiments by which I endeavoured, at the last meeting of the society, to evince the extraordinary effort of the air's resistance against shells and bullets discharged with their usual celerities; I should hope, that, what I have asserted on that head in my last paper, may be allowed to be demonstrated; and that I may now proceed to the examination of the effects of this resistance, without being accused of employing my speculations on a power that had no existence.

After the experiments I have shewn to this society, it may, perhaps, appear strange, that the persuasion of the minuteness of the air's resistance could so long take place, when the means of confuting this persuasion lay so near at hand; and were in a great measure to be deduced from the common laws of resistance, long since established. The most natural reason, I can think of, for this want of attention, was the impossibility of computing the motions of projectiles on the supposition of a sensible resistance. It is well known, that the motion of a body projected in any angle with the horizon, and resisted in the duplicate proportion of its velocity, was not considered by Sir *Isaac Newton* in his Principia. It is true, Mr.

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John Bernoulli did some years since determine, how by the quadrature of certain curves this motion might be assigned. But those, who are acquainted with his solution, well know, that those curves are not reducible to the common terms of quadratures; and, consequently, that no computation can be founded thereon; at least nothing of that kind has yet been published*. Now whilst the description of the real track described by a projectile continued thus impracticable, a mathematician might, perhaps, have thought it beneath him to have asserted, that the resistance of the air produced great changes in the motion of projected bodies, without having it in his power to assign the quantity of those changes.

But as I have, for some time past, made many experiments myself on the ranges of bullets, and have collected all that I could meet with made by other persons; it was necessary, in order to examine the several hypotheses of resistance, which some of these experiments suggested, that I should be enabled to compute the motions of resisted bodies, not only when they were resisted in the duplicate proportion of their velocity; but likewise when the law of resistance was varied by other rules not hitherto supposed by any writer. And, in these investigations, I had the good fortune to discover some compendious approximations, which were as accurate, as the nature of the subject required, and were as easy in their application, as I could

* The learned commentators upon Sir *Isaac Newton's* Principia, in their comment printed at *Geneva* in the year 1740, avow this truth; for, having given *Bernoulli's* solution of this problem, and discussed it at large, they add, "*Ex quibus manifestum sit "verae trajectoriae descriptionem adeò perplexam esse, ut ex illa "vix quidquam ad usus philosophicos aut mechanicos accommoda-"tum possit deduci.*" That is, "The description of the curve, "in which a projectile moves, is so very perplexed, that it can "be scarcely expected, any deduction should be thence made, "which may be adapted either to philosophical or mechanical "purposes." *Vide* vol. 2. p. 118.

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could well hope for in so perplexed and intricate a matter; and though many of these methods appear now to be useless, as I have found several of the laws of resistance, which they were fitted to examine, to be fictitious: yet, as some of these compendiums are applicable to the real motions of projected bodies, I shall insert such of them, as from their general form are the soonest described and the easiest to be remembered, and which are sufficient for computing the motions of shells and bullets in every practical operation of gunnery. But first it is necessary to examine what is the real law of resistance of bodies moving through the air.

I have already mentioned, that, in very great changes of velocity, the resistance does not accurately follow the duplicate proportion of the velocity. But how much this variation amounts to, and how it is adapted to the different velocities of the resisted body, it is not easy nicely to ascertain. However, by comparing together a great number of experiments; I am of opinion, that till a more accurate theory of these changes is completed, the two following positions may be assumed without any remarkable error.

First, That till the velocity of the projectile surpasses that of 1100 feet in a second, the resistance may be esteemed to be in the duplicate proportion of the velocity; and its mean quantity may be taken to be nearly the same with that, I have assigned in the former paper*.

Second,

* These suppositions are not nearly correct. In fact, by more accurate experiments with cannon-balls, it appears that the law of the resistance begins to increase above the ratio of the square of the velocity, from the very slowest motions, and thence goes on increasing gradually more and more above what is assigned by that ratio, till we arrive at the velocity of 1600 or 1700 feet per second, where it is at the greatest, amounting in that maximum state to only $2\frac{1}{10}$ times the quantity resulting from the ratio of the square of the velocity. And at the velocity of 1100

 feet,

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Second, That if the velocity be greater than that of 11 or 1200 feet in a second, then the absolute quantity of that resistance in these greater velocities will be near three times as great, as it should be by a comparison with the smaller velocities*.— For instance, the resistance of a 12 pound shot, moving with a velocity of 1700 feet in a second, instead of 144lb.$\frac{1}{2}$, which I have assigned it in a former paper, will be now three times that quantity, or 433lb.$\frac{1}{2}$.

Hence then it appears, that, if a projectile begins to move with a velocity less than that of 1100 feet in 1″; its whole motion may be supposed to be considered on the hypothesis of a resistance in the duplicate ratio of the velocity. And, if it begins to move with a velocity greater than this last mentioned, yet if the first part of its motion, till its velocity be reduced to near 1100 feet in 1″, be considered separately from the remaining part, in which the velocity is less than 1100 feet in 1″, it is evident, that both parts may be truly assigned on the

feet, instead of answering to that law, it amounts to 1.86 times the same. In short, the rule cannot be reduced to two cases only, as here supposed, but must have a different rate for every different velocity, according to the numbers in table 2 of my Dictionary, vol. 2, p. 365. H.

* As I have forbore to mix any hypothesis with the plain matters of fact deduced from experiment, I did not therefore animadvert on this remarkable circumstance, that the velocity, at which the moving body shifts its resistance, is nearly the same, with which sound is propagated through the air. Indeed, if the treble resistance in the greater velocities is owing to a vacuum being left behind the resisted body, it is not unreasonable to suppose, that the celerity of sound is the very least degree of celerity, with which a projectile can form this vacuum, and can in some sort avoid the pressure of the atmosphere on its hinder parts. It may perhaps confirm this conjecture to observe, that, if a bullet, moving with the celerity of sound, does really leave a vacuum behind it, the pressure of the atmosphere on its fore part is a force about three times as great as its resistance, computed by the laws observed in slow motions. But the exact manner, in which the greater and lesser resistances shift into each other, must be the subject of farther experimental enquiries.

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he same hypothesis, only the absolute quantity of the resistance is three times greater in the first part than in the last. Wherefore if the motion of a projectile on the hypothesis of a resistance in the duplicate ratio of the velocity be truly and generally assigned, the actual motions of resisted bodies may be thereby determined, notwithstanding the increased resistances in the great velocities. And, to avoid the division of the motion into two, I shall hereafter shew how to compute the whole at one operation with little more trouble, than if no such increased resistance took place.

To avoid frequent circumlocutions, the distance, to which any projectile would range in a vacuum on the horizontal plain at 45° of elevation, I shall call the potential random of that projectile.

And the distance to which the projectile would range in a vacuum on the horizontal plain at any angle different from 45°, I shall call the potential range of the projectile range at that angle.

And the distance to which a projectile really ranges, I shall call its actual range.

If the velocity, with which a projectile begins to move, is known; its potential random and its potential range at any given angle are easily determined from the common theory of projectiles:* or, more

* The method of computing the potential random from the known velocity of the projectile is thus. Find out by the theory of falling bodies, what height the projectile must fall from to acquire the given velocity, then twice this height is the potential random sought.

EXAMPLE.

Let the given velocity of the projectile be that of 1000 feet in 1″; then, since it is known, that a heavy body in falling 16 feet 1 inch, acquires a velocity of 32 feet 2 inches in 1″, and the spaces fell through are in a duplicate proportion of the velocities acquired by the fall; it follows, that the descent for

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more generally, if either its original velocity, its potential random, or its potential range, at a given angle are known, the other two are easily found out.

To facilitate the computation of resisted bodies it is necessary, in the consideration of each resisted body, to assign a certain quantity, which I shall denominate F, adapted to the resistance of that particular projectile. To find this quantity F to any projectile given, we may proceed thus; first find, from the principles delivered in the former paper, with what velocity the projectile must move, so that its resistance may be equal to its gravity. Then the height, from whence a body must descend in a vacuum to acquire this velocity, is the magnitude of F sought. But the concisest way of finding this quantity F to any shell or bullet is this: if it be of solid iron, multiply its diameter measured in inches by 300, the product will be the magnitude of F expressed in yards. If, instead of a solid iron bullet, it is a shell or a bullet of some other substance; then, as the specific gravity of iron is to the specific gravity of the shell or bullet given, so is the F corresponding to an iron bullet of the same diameter, to the proper F for the shell or bullet given. The quantity F being thus assigned, the necessary computations of these resisted motions may be dispatched by the three following propositions, always remembering that these propositions proceed on the hypothesis of the resistance being

acquiring a velocity of 1000 feet in a second is $\frac{1000^2}{64\frac{1}{4}}=15544$ feet, which, doubled, gives 31088 feet or 10363 yards for the potential random sought. And, in the same manner, if the potential random be given, the velocity of the projectile may be found. For the number of feet, or half the potential random, multiplied by $64\frac{1}{4}$, and the square root of the product extracted, that root will express the feet the projectile moves in a second. However, shorter methods of solving these cases, and what are sufficiently exact for our purpose, will be explained in the 4th paper hereafter inserted.

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being in the duplicate proportion of the velocity of the resisted body. How to apply this principle, when the velocity is so great, as to have its resistance augmented beyond this rate, shall be shewn in a corollary to be annexed to the first proposition.

PROP. I.

Given the actual range of a given shell or bullet at any small angle not exceeding 8° or 10°, to determine its potential range, and consequently its potential random and original velocity.

SOLUTION.

Let the actual range given be divided by the F corresponding to the given projectile, and find the quote in the first column of the annexed table. Then the corresponding number in the second column multiplied into F, will be the potential range sought; and thence, by the methods already explained, the potential random, and the original velocity of the projectile is given.

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Actual ranges expressed in F.	Corresponding potential ranges expressed in F.	Actual ranges expressed in F.	Corresponding potential ranges expressed in F.	Actual ranges expressed in F.	Corresponding potential ranges expressed in F.
0,01	0,0100	1,5	2,6422	3,25	13,2556
0,02	0,0201	1,55	2,7890	3,3	13,8258
0,04	0,0405	1,6	2,9413	3,35	14,4195
0,06	0,0612	1,65	3,0994	3,4	15,0377
0,08	0,0822	1,7	3,2635	3,45	15,6814
0,1	0,1034	1,75	3,4338	3,5	16,3517
0,12	0,1249	1,8	3,6107	3,55	17,0497
0,14	0,1468	1,85	3,7944	3,6	17,7768
0,15	0,1578	1,9	3,9851	3,65	18,5341
0,2	0,2140	1,95	4,1833	3,7	19,3229
0,25	0,2722	2,	4,3890	3,75	20,1446
0,3	0,3324	2,05	4,6028	3,8	21,0006
0,35	0,3947	2,1	4,8249	3,85	21,8925
0,4	0,4591	2,15	5,0557	3,9	22,8218
0,45	0,5258	2,2	5,2955	3,95	23,7901
0,5	0,5949	2,25	5,5446	4,0	24,7991
0,55	0,6664	2,3	5,8036	4,05	25,8506
0,6	0,7404	2,35	6,0728	4,1	26,9465
0,65	0,8170	2,4	6,3526	4,15	28,0887
0,7	0,8964	2,45	6,6435	4,2	29,2792
0,75	0,9787	2,5	6,9460	4,25	30,5202
0,8	1,0638	2,55	7,2605	4,3	31,8138
0,85	1,1521	2,6	7,5875	4,35	33,1625
0,9	1,2436	2,65	7,9276	4,4	34,5686
0,95	1,3383	2,7	8,2813	4,45	36,0346
1,0	1,4366	2,75	8,6492	4,5	37,5632
1,05	1,5384	2,8	9,0319	4,55	39,1571
1,1	1,6439	2,85	9,4300	4,6	40,8193
1,15	1,7534	2,9	9,8442	4,65	42,5527
1,2	1,8669	2,95	10,2752	4,7	44,3605
1,25	1,9845	3,0	10,7237	4,75	46,2460
1,3	2,1066	3,05	11,1904	4,8	48,2127
1,35	2,2332	3,1	11,6761	4,85	50,2641
1,4	2,3646	3,15	12,1816	4,9	52,4040
1,45	2,5008	3,2	12,7078	4,95	54,6363
				5,0	56,9653

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EXAMPLES.

An eighteen pounder, the diameter of whose shot is about 5 inches, when loaded with 2lb. of powder ranged at an elevation of 3° 30′ to the distance of 975 yards.

The F corresponding to this bullet is 1500 yards, and the quote of the actual range by this number is 65, corresponding to which in the second column is ,817, whence ,817 F, or 1225 yards, is the potential range sought, and this augmented in the ratio of the sine of twice the angle of elevation to the radius, gives 10050 yards for the potential random; whence it will be found, that the velocity of this projectile was that of 984 feet in a second.

COROLLARY 1st.

If the converse of this proposition be desired; that is, if the potential range in a small angle be given, and thence the actual range be sought; this may be solved with the same facility by the same table. For if the given potential range be divided by its correspondent F, then, opposite to the quote sought in the second column, there will be found in the first column a number, which multiplied into F will give the actual range required. And from hence it follows, that, if the actual range be given at one angle, it may be found at every other angle not exceeding 8° or 10°.

COROLLARY 2d.

If the actual range at a given small angle be given, and another actual range be given, to which the angle is sought; this will be determined by finding the potential ranges corresponding to the two given actual ranges; then the angle corresponding

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corresponding to one of these potential ranges being known, the angle corresponding to the other will be found by the common theory of projectiles.

COROLLARY 3d.

If the potential random reduced from the actual range by this proposition exceeds 13000 yards; then the original velocity of the projectile was so great as to be affected by the treble resistance described above; and consequently the real potential random will be greater than what is here determined. However, in this case the true potential random may be thus nearly assigned. Take a 4th continued proportional to 13000 yards, and the potential random found by this proposition, and the 4th proportional thus found, may be assumed for the true potential random sought. In like manner, when the true potential random is given greater than 13000 yards, we must take two mean proportionals between 13000 and this random:* and the first of these mean proportionals must be assumed instead of the random given, in every operation described in these propositions and their corollaries. And this method will nearly allow for the increased resistance in large velocities, the difference only amounting to a few minutes in the angle of direction of the projected body, which, provided that angle exceeds two or three degrees, is usually scarce worth attending to.

Of this process take the following example.

A twenty-four pounder fired with twelve pounds of powder, when elevated at 7° 15′, ranged about 2500 yards; here the F being near 1700 yards, the quote to be sought in the first column is 147, to which

* It may perhaps be unnecessary to observe, that the operations directed in this corollary are best performed by the table of logarithms.

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which the number corresponding in the second column is 2,556; whence the potential range is near 4350 yards, and the potential random thence resulting 17400; but this being more than 13000, we must, to get the true potential random, take a 4th continued proportional to 13000 and 17400, and this 4th proportional, which is about 31000 yards, is to be esteemed the true protential random sought; whence the velocity is nearly that of 1730 feet in a second.

SCHOLIUM.

This proposition is confined to small angles, not exceeding 8° or 10°, for reasons, which will be seen hereafter, when I give the demonstration of this and the ensuing problems. A subject, which being purely geometrical, I chuse to discuss in a small tract by itself. In all possible cases of practice, this approximation, thus limited, will not differ from the most rigorous solution by so much, as what will often intervene from the variation of the density of the atmosphere in a few hours time; so that the errors of the approximation are much short of other inevitable errors, which arise from the nature of this subject.

PROP. II.

Given the actual range of a given shell or bullet, at any angle not exceeding 45°, to determine its potential range at the same angle; and thence its potential random and original velocity.

SOLUTION.

Diminish the F corresponding to the shell or bullet given in the proportion of the radius to the cosine of $\frac{1}{4}$ of the angle of elevation. Then, by

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means of the preceding table, operate with this reduced F in the same manner as is prescribed in the solution of the last proposition, and the result will be the potential range sought; whence the potential random, and the original velocity, are easily determined.

EXAMPLE.

A mortar for sea-service, charged with 30lb. of powder, has sometimes thrown its shell of $12\frac{1}{2}$ inches diameter, and of 231lb. weight, to the distance of 2 miles or 3450 yards. This at an elevation of 45°.

The F to this shell, if it were solid, is 3825 yards; but as the shell is only $\frac{4}{7}$ of a solid globe, the true F is no more than 3060 yards. This, diminished in the ratio of the radius to the cosine of $\frac{1}{4}$ of the angle of elevation, becomes 2544. The quote of the potential range by this diminished F is 1,384, which sought in the first column of the preceding table, gives 2,280 for the corresponding number in the second column; and this multiplied into the reduced F, produces 5800 yards for the potential range sought; which, as the angle of elevation was 45°, is also the potential random: and hence the original velocity of this shell appears to be that of about 748 feet in a second.

COROLLARY.

The converse of this proposition, that is, the determination of the actual range from the potential range given, is easily deduced from hence, by means of the quote of the potential range divided by the reduced F; for this quote searched out in the second column will give a corresponding number in the first column, which multiplied into the reduced F, will be the actual range sought.

Also, if the potential random of a projectile be given, or its actual range at a given angle of elevation;

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vation; its actual range at any other angle of elevation, not greater than 45°, may hence be known. For the potential random will assign the potential range at any given angle, and thence, by the method of this corollary, the actual range may be found.

EXAMPLE.

A fit musquet bullet fired from a piece of the standard dimensions, with $\frac{1}{7}$ of its weight in good powder, acquires a velocity of near 900 feet in a second; that is, it has a potential random of near 8400 yards. If now the actual range of this bullet at 15° was sought, we must proceed thus.

From the given potential random it follows, that the potential range at 15° is 4200 yards, the diameter of the bullet is $\frac{3}{4}$ of an inch, and thence, as it is of lead, its proper F is 337,5 yards, which, reduced in the ratio of the radius to the cosine of $\frac{3}{4}$ of 15°, becomes 331 yards. The quote of 4200 by this number is 12,7 nearly, which, being sought in the second column, gives 3,2 nearly for the corresponding number in the first column; and this multiplied into 331 yards (the reduced F) makes 1059 yards for the actual range sought.

EXAMPLE II.

The same bullet, fired with its whole weight in powder, acquires a velocity of about 2100 feet in a second, to which there corresponds a potential random of about 45700 yards. But this number greatly exceeding 13000 yards, it must be reduced by the method described in the third corollary of the first proposition, when it becomes 19700 yards. If now the actual range of this bullet at 15° was required, we shall from hence find, that the potential range at 15° is 9850 yards, which, divided by the reduced F of the last example, gives for a

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quote 2975; and thence, following the steps prescribed above, the actual range of this bullet comes out 1396 yards, exceeding the former range by no more than 337 yards; whereas the difference between the two potential ranges is above ten miles. Of such prodigious efficacy is the resistance of the air, which hath been hitherto treated as too insignificant a power to be attended to in laying down the theory of projectiles.

SCHOLIUM.

The demonstration of the methods I have hitherto explained, for computing the motions of resisted bodies, I propose to refer to the geometrical part; where the principles and limits of these approximations will be discussed. But I must here observe, that as the density of the atmosphere perpetually varies, increasing and diminishing often by $\frac{1}{100}$ part, and sometimes more, in a few hours; for that reason I have not been over rigorous in forming these rules, but have considered them as sufficiently exact, when the errors of the approximation do not exceed the inequalities, which would take place by a change of $\frac{1}{100}$ part in the density of the atmosphere. With this restriction, the rules of this proposition may be safely applied in all possible cases of practice. That is to say, they will exhibit the true motions of all kinds of shells, and cannon-shot, as far as 45° of elevation, and of all musket-bullets fired with their largest customary charges, if not elevated more than 30°. Indeed, if experiments are made with extraordinary quantities of powder, producing potential randoms greatly surpassing the usual rate; then in large angles some farther modifications may be necessary. And though, as these cases are beyond the limits of all practice, it may be thought unnecessary to consider them; yet, to enable those who

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who are so disposed, to examine these uncommon cases; I shall here insert a proposition, which will determine the actual motion of a projectile at 45°, how enormous soever its original velocity may be. But as this proposition will rather relate to speculative than practical cases, instead of supposing the actual range known, thence to assign the potential random, I shall now suppose the potential random given, and the actual range to be thence investigated.

PROP. III.

Given the potential random of a given shell or bullet; to determine its actual range at 45°.

SOLUTION.

Divide the given potential random by the F corresponding to the shell or bullet given, and call the quotient q, and let l be the difference between the tabular logarithms of 25 and of q, the logarithm of 10 being supposed unity; then the actual range sought is $3,4\ F \mp 2lF - \frac{ll}{10} F$, where the double sign of 2lF is to be thus understood; that if q be less than 25, it must be — 2lF; if it be greater, then it must be + 2lF. In this solution q may be any number not less than 3, nor more than 2500.

COROLLARY.

Computing in the manner here laid down, we shall find the relation between the potential randoms, and the actual range at 45°, within the limits of this proposition, to be as expressed in the following table.

N Potential

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Potential Random.	Actual Range at 45°.
3 F	1,5 F
6 F	2,1 F
10 F	2,6 F
20 F	3,2 F
30 F	3,6 F
40 F	3,8 F
50 F	4,0 F
100 F	4,6 F
200 F	5,1 F
500 F	5,8 F
1000 F	6,4 F
2500 F	7,0 F

Whence it appears, that, when the potential random is increased from 3F to 2500F, the actual range is only increased from $1\frac{1}{2}$ F to 7 F; so that an increase of 2497F in the potential random, produces no greater an increase in the actual range than $5\frac{1}{2}$ F, which is not its $\frac{1}{400}$ part; and this will again be greatly diminished on account of the increased resistance, which takes place in great velocities. So extraordinary are the effects of this resistance, which we have been hitherto taught to regard as inconsiderable.

That the justness of the approximations laid down in the 2d and 3d propositions may be easier examined; I shall conclude these computations by inserting a table of the actual ranges at 45° of a projectile, which is resisted in the duplicate proportion of its velocity. This table is computed by methods different from those hitherto described, and is sufficiently exact to serve as a standard, with which the result of our other rules may be compared. And since whatever errors occur in the application of the preceding propositions, they will be most sensible at 45° of elevation, it follows, that hereby the utmost limits of those errors may be assigned.

Potential

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Potential Randoms.		Actual Range at 45°.	
,1	F	,0963	F
,25	F	,2282	F
,5	F	,4203	F
,75	F	,5868	F
1,0	F	,7323	F
1,25	F	,860	F
1,5	F	,978	F
1,75	F	1,083	F
2,0	F	1,179	F
2,5	F	1,349	F
3,0	F	1,495	F
3,5	F	1,624	F
4,0	F	1,738	F
4,5	F	1,840	F
5,0	F	1,930	F
5,5	F	2,015	F
6,0	F	2,097	F
6,5	F	2,169	F
7,0	F	2,237	F
7,5	F	2,300	F
8,0	F	2,359	F
8,5	F	2,414	F
9,0	F	2,467	F
9,5	F	2,511	F
10,0	F	2,564	F
11,0	F	2,651	F
13,0	F	2,804	F
15,0	F	2,937	F
20,0	F	3,196	F
25,0	F	3,396	F
30,0	F	3,557	F
40,0	F	3,809	F
50,0	F	3,998	F

Thus I have dispatched all I thought necessary to insert in this paper, relating to the computations of the actual motions of shells and bullets. It might now perhaps be expected, that I should exemplify

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emplify these rules by applying them to the examination of a number of experiments made with various kinds of artillery at different angles. But besides the doctrine of the air's resistance, to which this paper hath been hitherto confined; the theory, which I have undertaken to establish, consists of another branch not less important: I mean the consideration of the action of fired powder, and the assigning from thence the velocity and potential random of a given bullet impelled by a given charge. As therefore the experiments, I shall hereafter produce, are adapted to the confirmation of both branches of the theory; the discussion of them must be of course postponed, till the doctrine of the action of powder be explained. And that is too copious a subject to be confined within the limits of this paper; especially too as it is extremely necessary, before any other steps are taken on the present subject, to explain distinctly the irregularities, which frequently occur in the flight of projectiles, and which divert them so wonderfully from their assigned track. This, though hitherto unobserved, is a matter of very great import in the theory of the motions of military projectiles. And I shall here endeavour to discuss it with as much conciseness as I can.

Almost every projectile, besides the forces we have hitherto considered, that is, its gravitation, and that resistance of the air which directly opposes its motion, is affected by a third force, which acts obliquely to its motion, and in a variable direction; and which consequently deflects the projectile from its regular track, and from the vertical plane, in which it began to move; impelling it sometimes to one side, and sometimes to the other, occasioning thereby very great inequalities in the repeated ranges of the same piece, though each time loaded and pointed in the same manner; and this force operating thus irregularly I conceive to

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be the principal source of all that uncertainty and confusion in the art of gunnery, which hath hitherto been usually ascribed to the difference of powder. The reality of this force, and the cause which produces it, will, I hope, appear from the following considerations.

It will easily be granted, I suppose, that no shell or bullet can be discharged from the pieces generally in use, without rubbing against their sides, and thereby acquiring a whirling motion, as well as a progressive one. And as this whirl will in one part of its revolution conspire in some degree with the progressive motion, and in another part be equally opposed to it; the resistance of the air on the fore part of the bullet will be hereby affected, and will be increased in that part where the whirling motion conspires with the progressive, and diminished where it is opposed to it. And by this means the whole effort of the resistance, instead of being in a direction opposite to the direction of the body, will become oblique thereto, and will produce those effects we have already mentioned. If it was possible to predict the position of the axis, round which the bullet should whirl, and if that axis was unchangeable during the whole flight of the bullet, then the abberration of the bullet by this oblique force would be in a given direction, and the incurvation produced thereby would regularly extend the same way, from one end of its track to the other. For instance, if the axis of the whirl was perpendicular to the horizon; then the incurvation would be to the right or left. If that axis were horizontal and perpendicular to the direction of the bullet; then the incurvation would be upwards or downwards. But as the first position of this axis is uncertain, and as it may perpetually shift in the course of the bullet's flight, the deviation of the bullet is not necessarily either in one certain direction, nor tending to the same side

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in one part of its track that it does in another, but it more usually is continually changing the tendency of its deflection, as the axis, round which it whirls, must frequently shift its position to the progressive motion by many inevitable accidents.

That a bullet generally acquires such a rotatory motion, as I have here described, and that these irregularities are the necessary consequences of such a rotation, is, I think, demonstrable. However, as in these novel assertions, I would leave no room for doubt or dispute, I propose first to shew to the society, by an obvious experiment*, that the direction of a ball will be sensibly changed by compounding its progressive motion with a revolving one, although both these motions, and consequently their effects, are prodigiously short of what must necessarily take place in military projectiles; and I next propose to exhibit to any gentlemen, who will honour me with their company, an ocular proof of the deflection of musket bullets, even in the small interval of 60, 70, or 100 yards. It is true, in larger intervals these inequalities are much more considerable; for I have seen a bullet in a range of 700 yards deviate above 100 yards from its direction. But as proper ground sufficient for these extended trials is not to be met with in the neighbourhood of *London*, I hope the experiments I shall make in a shorter space, as they will evince the reality of these deflections, will procure belief to other experiments where they have been much greater. What methods are necessary for the avoiding of these divarications, I may perhaps treat of in the course of some future papers on the subject of gunnery.

I shall only add, that, as it may be imagined, that the effects of the oblique resistance, I have here described, can be nicely ascertained by the common

* This experiment, with all the others mentioned in this and the next paragraph, are described in the following paper.

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common established doctrine of the action of fluids on a surface placed oblique to their motions: I must therefore observe (to prevent any persons from wasting their time in so fruitless an examination) that all the positions hitherto laid down by any author whatever, about the comparative resistances of oblique surfaces, are most egregiously erroneous. It would too much enlarge the present paper to demonstrate this in every instance; I shall therefore content myself with exhibiting one experiment, which, if properly weighed, will, I believe, render that trouble unnecessary. In this experiment I shall shew, that two equal surfaces, each of them meeting the air with the same degree of obliquity, are yet so differently resisted, that though in one the resistance be less than that of a perpendicular surface meeting the same quantity of air, yet in the other it is greater. But on this important subject (the compleating of which is absolutely necessary to the establishing the true theory of ship-building and sailing) I hope to be more explicit hereafter.

No. III.

An Account of the Experiments, relating to the Resistance of the Air, exhibited at different Times before the Royal Society, in the Year 1746.

In the latter end of the last summer, I delivered in two papers to the society on the resistance of the air: the first endeavouring to demonstrate the very great effects thereby produced in the motion of almost every military projectile, and consequently the necessity of considering it in all speculations upon the subject of gunnery; the other containing a more accurate and minute explication of the laws observed by this resistance, together with rules for computing the actual ranges of projectiles as influenced and regulated thereby. And, to compleat this subject, there was inserted, in this last paper, a description of an extraordinary and hitherto unsuspected irregularity, to which these motions are liable; with a discussion of the causes of the disturbing force, by which it is brought about. And hence occasion was taken to add, in the conclusion, some very paradoxical assertions relating to the difference, that is found in the resistance of the air to the same oblique surface, only by varying of such circumstances as have hitherto been supposed to have no connexion with that resistance.

The propositions advanced in these papers, being most of them directly contrary to the opinions generally received on this subject; their proof depended on certain experiments, which I had myself

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self frequently repeated, and which, upon this occasion, I exhibited to the society. And as they were often reiterated before numbers of the members, and always with sufficient success; I think it incumbent on me to describe the machines and methods made use of on this occasion, together with an account of the nature and events of the trials thus exhibited. For I conceive a narration of this kind, corroborated by the recollection of those gentlemen who were then present, will be a much stronger confirmation of the principles, I advance, than the result of any experiments, which depended merely on my own relation. For my single evidence might perhaps be considered as precarious; as I might at least be suspected of being biassed towards those novel doctrines, of which I profess myself in some degree the inventor.

To begin then with this narration, it is necessary to mention, that, in the first paper delivered to the society, I asserted the two following propositions:

First, "That the resistance of the air to a 12lb. "iron bullet, moving with a velocity of 25 feet in "a second, is not less than half an ounce avoirdu-"pois."

Secondly, "That the resistance of the air, "within certain limits, is nearly in the duplicate "proportion of the velocity of the resisted body."

These propositions are in themselves neither unknown nor doubtful; but yet, as they are the basis of some other assertions, which have been hitherto constantly contested and denied; I thought it requisite to evince their veracity by more unquestioned and simpler methods, than have been hitherto practised: for this purpose, I therefore caused a machine to be made of the form represented in the annexed drawing*, which was most excellently executed under the direction of Mr. *Ellicot*,

* Plate II. Fig. I.

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Ellicot, and was completely fitted for the use intended by many contrivances, some of them not contained in the drawing, nor necessary to be particularized in this place. For, it is sufficient, for the purpose of the following experiments, to describe its general fabric, by observing that BCDE is a brass barrel moveable on its axis, and so adjusted by means of friction wheels, which are not represented in the drawing, as to have no friction worth attending to. The frame, in which this barrel is fixed, is so placed, that its axis may be perpendicular to the horizon. The axis itself is continued above the upper plate of the frame, and has fastened on it a light hollow cone AFG; from the lower part of this cone there is extended a long arm of wood GH, which is very thin, and cut feather-edged, and at its extremity there is a contrivance for fixing on the body, whose resistance is to be investigated (as here the globe P) and, to prevent the arm GH from swaying out of its horizontal position by the weight of the annexed body P, there is a brace AH of fine wire fastened to the top of the cone, which supports the end of the arm.

Round the barrel BCDE there is wound a fine silk line, the turns of which appear in the figure, and after this line hath taken a sufficient number of turns, it is conducted nearly horizontally to the pully L, over which it is passed, and then a proper weight M is hung to its extremity. If this weight be left at liberty, it is obvious, that it will descend by its own gravity, and will by its descent turn round the barrel BCDE, together with the arm GH and the body P fastened to it. And whilst the resistance on the arm GH and on the body P is less than the weight M, that weight will accelerate its motion, and thereby the motion of GH and P will increase, and consequently their resistance will increase, till at last this resistance and the

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the weight M become nearly equal to each other. The motion with which M descends, and with which the body P revolves, will not sensibly differ from an equable one. Whence it is not difficult to conceive, that by proper observations made with this machine. The resistance of the body P may be determined, the most natural method of proceeding in this investigation is as follows. Let the machine have first acquired its equable motion (which, as will hereafter appear, will be usually attained in five or six turns from the beginning) and then let it be observed, by counting a number of turns, what time is taken up by one revolution of the body P; then taking off the body P and the weight M, let it be examined, what smaller weight will make the arm GH revolve in the same time, as when P was fixed to it; this smaller weight being taken from M, the remainder is obviously equal in effort to the resistance of the revolving body P; and this remainder being reduced in the ratio of the length of the arm to the semidiameter of the barrel, will then become equal to the absolute quantity of the resistance. And as the time of one revolution is known, and consequently the velocity of the revolving body; there is thereby discovered, the absolute quantity of the resistance to the given body P, moving with a given degree of celerity.

And note, that to avoid all exceptions, I have generally chose, when the body P was removed, to fix in its stead a thin piece of lead of the same weight, placed horizontally; so that the weight, which was to turn round the arm GH without the body P, did also carry round this piece of lead. This I did, lest it should be objected, that the body P retarded the weight M by its quantity of matter, as well as by its resistance. But mathematicians will easily allow, that there was no necessity for this precaution.

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The measures of the parts of this machine were, as follows:

	Inches.
The diameter of the barrel BCDE and of the silk string wound round it was	2,06
The length of the arm GH measured from the axis to the surface of the globe P was	49,5
The body P, the globe made use of was of pasteboard, its surface very neatly coated with marble paper. It was not much distant from the size of a 12lb. shot, being in diameter	4,5
So that the radius of the circle described by the centre of the globe was	51,75

When this globe was fixed at the end of the arm, and a weight of half a pound was hung at the end of the string at M; it was examined, how soon the motion of the descending weight M, and of the revolving globe P, would become equable as to sense. And with this view three revolutions being suffered to elapse, it was found, that

The next 10 were performed in	$27''\frac{3}{4}$
20 in less than	55
30 in	$82\frac{1}{2}$
So that the first 10 were performed in	$27''\frac{3}{4}$
2d 10 in	$27\frac{1}{4}$
3d 10 in	$27\frac{1}{4}$

These experiments sufficiently evince, that even with half a pound, the smallest weight hereafter used, the motion of the machine was sufficiently equable after the first three revolutions.

And now to prove the two forementioned propositions, the following experiments were made; the times marked down being observed by several stop-watches, which rarely differed half a second from each other.

The forementioned globe being fixed at the end of the arm, there was hung on in the situation M a weight of 3lb.$\frac{1}{4}$, and 10 revolutions being suffered to

to elapse, the succeeding 20 were performed in $21''\frac{1}{2}$.

Then the globe being taken off, and a thin plate of lead, equal to it in weight, placed in its room; it was found, that, instead of 3lb.$\frac{1}{4}$, a weight of 1lb. would make it revolve in less time, than it did before, it performing 20 revolutions, after 10 were elapsed, in the space of 19 seconds.

Hence then it follows, that from the 3lb.$\frac{1}{4}$ first hung on, there is less than 1lb. to be deducted for the resistance on the arm, and consequently the resistance on the globe itself is not less than the effort of 2lb.$\frac{1}{4}$ in the situation M; and it appearing from the former measures, that the radius of the barrel is nearly $\frac{1}{50}$ part of the radius of the circle described by the centre of the globe; it follows, that the absolute resistance of the globe, when it revolves 20 times in $21''\frac{1}{2}$ (which, if computed by the measures given above, comes out a velocity of about 25$\frac{1}{4}$ feet in a second) it follows, I say, that the resistance of the globe in this case is not less than the $\frac{1}{50}$ part of 2lb.$\frac{1}{4}$, or than the $\frac{1}{50}$ part of 36 ounces: and this being considerably more than half an ounce, and the globe being nearly the size of a 12 pounder; it irrefragably confirms our first proposition, "That the resistance of the air to a "12lb. iron bullet, moving with a velocity of 25 "feet in a second, is not less than half an ounce "avordupois."

The next experiments were made with a view of examining the 2d proposition. And for this purpose there were successively hung on in the situation M weights in the proportion of the numbers 1, 4, 9, 16; and letting 10 revolutions first elapse, the observations on those that follow, were as under.

With 1lb. the globe went	20 turns in	$54''\frac{1}{2}$
that is, it went	10 turns in	$27\frac{1}{4}$
With 2lb. it went	20 turns in	$27\frac{1}{2}$

With

With 4lb. $\frac{1}{2}$ it went	30 turns in	$27''\frac{1}{2}$
With 8lb. it went	40 turns in	$27\frac{1}{2}$

So that it appears, that to resistances of the proportions of the numbers 1, 4, 9, 16, there correspond velocities of the resisted body in the proportion of the numbers 1, 2, 3, 4, which proves with great nicety, within the limits of these experiments, the second proposition contained in our first paper, "That the resistance of the air is "nearly in the duplicate proportion of the velocity "of the resisted body. That is, it is four times as "much when the resisted body moves with twice "the velocity; nine times as much, when it moves "with three times the velocity, and so on."

These were the experiments exhibited to evince the assertions contained in the first paper, and these having been shewn at different times to numbers of the society, without any exceptions being made thereto, that I have yet heard of; they were taken for granted in my second paper, and considered as a full confirmation of what I had before advanced.

The second paper, though principally employed in giving rules for the computation of the motions of military projectiles, as modulated by the resistance of the air, did yet, in the latter part of it, contain some observations relating to the hitherto unheeded effects produced by this resistance; for its action is not solely employed in retarding the motions of projectiles, but some part of it exerted in deflecting them from their course, and in twisting them in all kinds of directions from their regular track; this is a doctrine, which, notwithstanding its prodigious import to the present subject, hath been hitherto entirely unknown, or unattended to; and therefore the experiments, by which I have confirmed it, merit, I conceive, a particular description; as they are themselves too of a very singular kind.

The

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The basis of this doctrine, which I have explained at large at the end of my second paper, but which it is necessary to describe in a few words in this place, is this; that almost all bullets receive a whirling motion by rubbing against the sides of the pieces, they are discharged from; and that this whirling motion of the bullet occasions it to strike the air obliquely, and thereby produces a resistance, which is oblique to the track of the bullet, and consequently perpetually deflects it from its course.

The first experiment, exhibited on this occasion, was to evince, that the whirling motion of a ball, combining with its progressive motion, would produce such an oblique resistance and deflective power, as is herein mentioned. For this purpose, a wooden ball, 4½ inches diameter, was suspended by a double string about eight or nine feet long. Now by turning round the ball, and twisting the double string, the ball, when left to itself, would have a revolving motion given it from the untwisting of the string again. And if, when the string was twisted, the ball was drawn a considerable distance from the perpendicular, and there let go; it would at first, before it had acquired its revolving motion, vibrate steadily enough in the same vertical plane, in which it first began to move; but when, by the untwisting of the string, it had acquired a sufficient degree of its whirling motion, it constantly deflected on the right or left of its first track; and sometimes proceeded so far, as to have its direction at right angles to that, in which it began its motion; and this deviation was not produced by the action of the string itself, but appeared to be entirely owing to the resistance being greater on the one part of the leading surface of the globe than on the other. For the deviation continued when the string was totally untwisted, and even during the time that the string, by the motion the globe had received, was twisting the

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contratry way. And it was always easy to predict, before the ball was let go, which way it would deflect, only by considering on which side the whirl would be combined with the progressive motion; for on that side always the deflecting power acted; as the resistance was greater here, than on the side where the whirl and progressive motion were opposed to each other.

This experiment is an incontestible proof, that, if any bullet, besides its progressive motion, hath a whirl round its axis; it will be deflected in the manner here described. And as it is scarcely possible to suppose, but that every bullet, discharged from the pieces now in common use, must receive such a whirl from its friction against the sides of the piece; the proposition might perhaps be safely rested on this single experiment. But not to leave any thing doubtful in a subject liable to so much contestation; I undertook to evince, by an ocular proof, the reality of this deflection in musquet-bullets, even in so short an interval as a hundred yards. And these experiments having succeeded to the general satisfaction of those who honoured me with their company; I shall here describe, as briefly as I can, the manner in which they were tried, and the conclusions resulting from them.

As all projectiles in their flight are acted on by the power of gravity, the deflexion of a bullet from its primary direction, supposes that deflexion to be upwards or downwards in a vertical plane; because, in the vertical plane, the action of gravity is compounded and entangled with the deflective force. And for this reason, my experiments have been principally directed to the examination of that deflexion, which carries the bullet to the right or left of the vertical plane, in which it began to move. For if it appears at any time, that the bullet has shifted from that vertical plane, in which its motion began; this will be an incontestable

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table confirmation of what we asserted. Since no other power but that unequal resistance, which we here insist on, can occasion a body in motion to deviate from the vertical plane, in which it has once moved.

Now by means of screens of exceeding thin paper, placed parallel to each other at proper distances, this deflection in question may be many ways investigated. For by firing bullets which shall traverse these screens, the flight of the bullet may be traced out; and it may easily appear, whether they do or do not keep invariably to one vertical plane. This examination may proceed on three different principles, which I shall here separately explain.

For first an exact vertical plane may be traced out upon all these screens, by which the deviation of any single bullet may be more readily investigated, only by measuring the horizontal distance of its trace from the vertical plane thus delineated, and by this means the absolute quantity of its aberration may be known.

Or if the description of such a verical plane should be esteemed a matter of difficulty and nicety, a second method may be followed, which is that of resting the piece in some fixed notch or socket, so that though the piece may have some little play to the right and left, yet all the lines, in which the bullet can de directed, shall intersect each other in the center of that fixed socket; by this means, if two different shot are fired from the piece thus situated, the horizontal distances of the traces made by the two bullets on any two screens, ought to be in the same proportion to each other as the respective distances of these screens from the socket, in which the piece was laid. And if these horizontal distances differ from that proportion; then it is certain, that one of these shot at least hath deviated from a vertical

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plane, although the absolute quantity of that deviation cannot be hence assigned; because it cannot be known, what part of it is to be imputed to one bullet, and what to the other.

But if the constant and invariable position of the notch or socket, in which the piece was placed, be thought too hard an hypothesis in this very nice affair; the third method, and which is the simplest of all, requires no more than, that two shot be fired through three screens, without any regard to the position of the piece each time. For, in this case, if the shots diverge from each other, and both keep to a vertical plane; then if the horizontal distances of their traces on the first screen be taken from the like horizontal distances on the second and third, the two remainders will be in the same proportion with the distances of the second and third screen from the first. And if they are not in this proportion, then it will be certain, that one of them at least hath been deflected from the vertical plane; though here, as in the last instance, the quantity of that deflexion in each will not be known.

All these three methods I have myself made use of at different times; and have ever found the success agreeable to my expectation. But what I thought the most eligible for the experiments, which I proposed to shew to the society, was a compound of the two last, and the apparatus was as follows:

On ——, being the first day appointed for these trials, the weather was unfavourable, and the experiments on that account more confused than could have been wished, though they were far from inconclusive. But on the next Thursday two screens were set up in the large walk in the Charter-house garden; the first of them at 250 feet distance from the wall (which was to serve for a third screen) and the second two hundred feet from the

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ame wall. And at fifty feet before the first screen, or at 300 feet from the wall, there was placed a large block, weighing about 200lb. weight, and having fixed into it an iron bar with a socket at its extremity, in which the piece was to be laid. The piece itself was of a common length, and was bored for an ounce ball. It was each time loaded with a ball of 17 to the pound, (so that the windage was extremely small) and with a $\frac{1}{4}$ of an ounce of good powder. The screens were made of the thinnest issue paper; and the resistance, they gave to the bullet (and consequently their probability of deflecting it) was so small, that a bullet lighting one time near the extremity of one of the screens, left a fine thin fragment of it towards the edge entire, which was so very weak, that it appeared difficult to handle it without breaking. These things thus prepared, five shot were made with the piece rested in the notch described above; and the horizontal distances between the first shot, which was taken as a standard, and the four succeeding ones, both on the first and second screen, and on the wall, measured in inches, were as follows:

	1st screen.	2d screen.	wall.
1 to 2	1,75 R	3,15 R	16,7 R
3	10, L	15,6 L	69,25 L
4	1,25 L	4,5 L	15,0 L
5	2,15 L	5,1 L	19,0 L

Here the letters R and L denote, that the shot in question went either to the right or left of the first.

If the position of the socket, in which the piece was placed, be supposed fixed, (and I presume no person then present conceived, during these trials, that it could possibly vary the 10th of an inch from its first situation) then the horizontal distances, measured above on the 1st and 2d screen, and on the wall, ought to be in the proportion of

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the distances, of the 1st screen, the 2d screen, and the wall, from the socket. But, by only looking over these numbers; it appears, that none of them are in that proportion. The horizontal distance of the 1st and 3d (for instance) on the wall being above 9 inches more, than it should be by this analogy.

If, without supposing the invariable position of the socket, we examine the comparative horizontal distances according to the third method described above; we shall in this case discover divarications still more extraordinary. For by the numbers set down it appears, that the horizontal distances of the 2d and 3d shot on the two screens, and on the wall, are as under:

1st screen	2d screen	wall
11,75	18,75	85,95

Here, if, according to the rule given above, the distance on the first screen be taken from the distances on the other two, the remainder will be 7, and 74,2; and these numbers, if each shot kept to a vertical plane, ought to be in the proportion of 1 to 5, that being the proportion of the distances of the second screen, and of the wall from the first. But the last number 74,2 exceeds what it ought to be by this analogy, by 39,2. So that between them there is a deviation from the vertical plane of above 39 inches; and this too in a transit of little more than 80 yards.

But further, to shew that these irregularities do not depend upon any accidental circumstances of the ball's fitting or not fitting the piece, there were five shots more made with the same quantity of powder as before; but with smaller bullets, which ran much looser in the piece. And the horizontal distances being measured in inches from the trace of the first bullet to each of the succeeding ones, the numbers were as follow;

1st screen

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	1st screen	2d screen	wall
1 to 2	15,6 R	31,1 R	94,0 R
3	6,4 L	12,75 L	23,0 L
4	4,7 R	8,5 R	15,5 R
5	12,6 R	24,0 R	63,5 R

Here again, on the supposed fixed position of the piece, the horizontal distance on the wall, between the first and third, will be found to be above 15 inches less, than it should be, if each kept to a vertical plane. And like irregularities, though smaller, occur in every other experiment. And if they are examined according to the third method set down above; and the horizontal distances of the third and fourth, for instance, are compared, these on the first and second screen, and on the wall, appear to be thus;

1st screen	2d screen	wall
11,1	21,25	38,5

And if the horizontal distance on the first screen be taken from the other two, the remainders will be 10,15 and 27,4; where the least of them, instead of being five times the first, as it ought to be, is 23,35 short of it. So that here there is a deviation of above 23 inches.

From all these experiments the deflexion in question seems to be incontestably evinced. But to give some farther light to this subject, I took a barrel of the same bore with that hitherto used; and bent it at about three or four inches from its muzzle to the left, the bend making an angle of 3° or 4° with the axis of the piece. This piece, thus bent, was fired with a loose ball, and the same quantity of powder hitherto used, the screens of the last experiment being still continued. It was natural to expect, that if this piece was pointed by the general direction of its axis; the ball would be canted to the left of that direction by the bend near its mouth. But as the bullet, in passing through that bent part, would, as I conceived, be

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forced to roll upon the right-hand side of the barrell, and thereby the left side of the bullet would turn up against the air, and would increase the resistance on that side; I predicted to the company then present, that if the axis, on which the bullet whirled, did not shift its position after it was separated from the piece, then, notwithstanding the bend of the piece to the left, the bullet itself might be expected to incurvate towards the right; and this, upon trial, did most remarkably happen. For one of the bullets fired from this bent piece, passed through the first screen about $1\frac{1}{2}$ inch distant from the trace of one of the shot fired from the straight piece in the last set of experiments. On the second screen the traces of the same bullets were about three inches distant, the bullet from the crooked piece passing on both screens to the left of the other; but comparing the places of these bullets on the wall, it appeared, that the bullet from the crooked piece, though it diverged from the track of the other on the two screens, had now crossed that track, and was deflected considerably to the right of it: so that it was obvious, that, though the bullet from the crooked piece might at first be canted to the left, and had diverged from the track of the other bullet, with which it was compared, yet by degrees it deviated again to the right, and a little beyond the second screen crossed that track, from which it before diverged; and on the wall was deflected 14 inches, as I remember, on the contrary side. And this experiment is not only the most convincing proof of the reality of this deflexion here contended for; but is likewise the strongest confirmation, that it is brought about in the very manner, and by the very circumstances, which we have all along described.

These were the experiments exhibited for the establishing of this new doctrine, and which, being

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being performed in this public manner, and so generally acquiesced in by those who were present, will, I hope, secure me from the harsh and malevolent censures, which propounders of new opinions are generally exposed to. This doctrine is, I confess, of so extensive an influence, and destroys such a multitude of theories, projects, and conclusions, with which the modern writers on the art of gunnery have abounded, that it will not be admitted but with the greatest caution and difficulty: but after the success of the experiments here recited (and they bear but a very small proportion to those, I have myself tried on the same occasion) it will not be easy, I conceive, to urge any exceptions against it, that shall have the air of validity. And I think, I am so much master of this subject, as to undertake the refutation of whatever objections shall be hereafter started upon this head. I have only now to add, that, as I suspected, the consideration of the revolving motion of the bullet, compounded with its progressive motion, might be considered as a subject of mathematical speculation, and that the reality of any deflecting force, thence arising, might perhaps be denied by some computists upon the principles hitherto received of the action of fluids: to prevent a too hasty discussion of this kind, I thought proper to annex a few experiments, with a view of evincing the strange deficiency of all theories of this sort hitherto established, and the unexpected and wonderful varieties, which occur in these matters. The proposition which I advanced for this purpose being, that two equal surfaces meeting the air with the same degree of obliquity, may be so differently resisted, that, though in one of them the resistance is less than that of a perpendicular surface meeting the same quantity of air, yet in the other it shall be considerably greater.

To make out this proposition, I made use of the

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machine described in the first part of this paper, and having prepared a pasteboard pyramid, whose base was four inches square, and whose planes made angles of 45° with the plane of its base; and also a parallelogram, four inches in breadth and $5\frac{7}{8}$ in length, which was equal to the surface of the pyramid. The globe P was taken off from the machine, and the pyramid was first fixed on; and 2lb. being hung at M, and the pyramid so fitted as to move with its vertex forwards, it performed 20 revolutions, after the first 10 were elapsed, in 33″.

Then the pyramid being turned so that its base, which was a plane of four inches square, went foremost; it now performed 20 revolutions with the same weight in $38''\frac{1}{4}$.

After this, taking off the pyramid, and fixing on the parallelogram with its longer side perpendicular to the arm, and placing its surface in an angle of 45° with the horizon by a quadrant, the parallelogram with the same weight performed 20 revolutions in $43''\frac{1}{2}$.

Now here this parallelogram and the surface of the pyramid are equal to each other, and each of them met the air in an angle of 45°, and yet one of them made 20 revolutions in 33″, whilst the other took up $43''\frac{1}{2}$. And at the same time it appears, that a flat surface (as the base of a pyramid) which meets the same quantity of air perpendicularly, makes 20 revolutions in $38''\frac{1}{4}$, which is the medium between the other two; whence the proposition advanced above is evinced.

But to give another proof of this principle, which is still simpler; there was taken a parallelogram 4 inches broad and $8\frac{1}{4}$ long; this being fixed at the end of the arm, with its longer side perpendicular thereto, and being placed in an angle of 45° with the horizon, there was a weight hung on at M of 3lb.$\frac{1}{2}$, with which the parallelogram made

20

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20 revolutions in $40''\frac{3}{4}$. But after this the position of the parallelogram was shifted, and it was placed with its shorter side perpendicular to the arm, though its surface was still inclined to an angle of 45° with the horizon. And now, instead of going slower, as might be expected, from the greater extent of part of its surface from the axis of the machine, it went round much faster. For in this last situation, it made 20 revolutions in $35''\frac{3}{4}$; so that here were $5''$ difference in the time of 20 revolutions, and this from no other change of circumstance, than as the larger or shorter side of the oblique plane was perpendicular to the line of its direction. As to the cause of this extraordinary inequality; it is not my business at present to enlarge, nor have I indeed as yet completed all the experiments, I have projected on this subject. However, thus far may be easily concluded, that all the theories of resistance hitherto established, are extremely defective, and that it is only by experiments analogous to those here recited, that this important subject can ever be completed: I say important subject; for surely a matter, on the right knowledge of which all true speculations on ship-building and sailing must necessarily depend, cannot but be deemed, in this country at least, of the highest importance, both to the public interest of the nation, and to the general benefit of mankind.

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No. IV.

Of the Force of fired Gunpowder, together with the Computation of the Velocities thereby communicated to Military Projectiles.

ALL, that hath been hitherto said in the preceding papers, hath principally related to the resistance of the air, and its effects upon the motions of military projectiles. But the theory of gunnery includes in it the knowledge of another matter not less important; I mean the theory of the action of fired powder, and the determination of the velocities communicated to bullets by its explosion.

And this last subject I have formerly treated very amply; and as, by all the experiments I have since made, I find no reason to change my opinion in any essential point, I shall here insert the substance of this doctrine in as concise a manner as I can, together with some rules deduced from it for computing the original velocities of bullets. And this article being dispatched, I shall, in a future paper, proceed to examine how far the motions of bullets assigned by the two theories, I have delivered, (that of the impulse of the powder, and that of the action of the air's resistance) are conformable to the experiments, which have been made in different places on the flight of shells and cannon-shot. It is true, very few of these experiments have been conducted with due attention, or have been instituted with proper views. However, as I have collected every thing of this kind, that hath at any time fallen in my way, and, as I conceive, the comparison even of the most irregular experiments with the theory will be far from disgracing it; I flatter myself, that this examination

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tion will not be totally void either of curiosity or utility. And, with regard to the present paper, I shall only mention, that all the propositions I am going to insert, on the action of fired powder, I propose to demonstrate by an experimental process, whenever a convenient season will permit me to exhibit it before the society.

PROP. I.

Gunpowder fired in any space, acts nearly in the same manner, as a quantity of air would do, which was condensed a thousand times more than the common air we breathe; and which, in that condensed state, filled the same space, that was taken up by the unfired powder.

The proof of this proposition depends on various experiments made on the velocities of bullets, at their first issuing from the piece. It is true, that different kinds of powder act with different degrees of force; but we suppose the powder here meant to be made by the government standard. I must add, too, that some varieties will take place in this proposition, according to the different quantity of powder made use of, and the manner in which it is placed. However, none of these things are of moment enough to be attended to in the forming a general theory; though they may hereafter, in a proper place, merit a more particular discussion.

PROP. II.

Hence it follows, that the pressure of the powder on the bullet, grows perpetually weaker and weaker, as the bullet is farther impelled before it.

For as the bullet is impelled forwards, the inflamed powder takes up more room; and consequently its elasticity is diminished. So that, for instance, if the charge of powder in a twenty-four pounder takes up one foot of the cylinder, before it

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it is fired, and the whole length of the cylinder be nine feet; then when the bullet arrives at the mouth of the piece, the powder extends through nine times the space, it did at first; and consequently exerts but $\frac{1}{9}$ of its original pressure; and the longer the piece is in proportion to the extent of the charge, the more is the action of the powder diminished.

PROP. III.

Though the velocity of the bullet constantly increases by the pressure of the powder, yet its acceleration, in passing through a given space, grows continually less and less, as it approaches the mouth of the piece; and this on a twofold account. For both the pressure of the powder decreases in the manner described in the last proposition, and the velocity acquired by the bullet renders the action of that pressure, in passing through a given space, less efficacious; for the faster the bullet moves, the less it is obviously accelerated, in passing through a given space, by the pressure of the powder, which follows it.

PROP. IV.

If the same piece be fired successively with different charges of powder, the pressure of these different charges upon the bullet, in any given part of the barrel, is nearly in proportion to the quantity in each charge.

This follows from the analogy established in the first proposition, between the action of powder and the action of condensed air; for it is well known, that the elasticity of air inclosed in a given space is in proportion to its quantity.

PROP. V.

From the last proposition it follows, that if with different charges the bullet or shell be always placed

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placed in the same part of the barrel (as is the case in mortars) then the collective pressure of various charges will be as their quantities of powder; and to this collective pressure the square of the velocity of the bullet will be proportional. But if the bullet be placed home upon the powder, so that with a smaller charge it is nearer the breech than with a larger; in this case the collective pressure of the smaller charge will be somewhat more than in proportion to its quantity; for it will act through a longer space than the larger charge; and in this additional space, as it lies contiguous to the first inflammation, the action will be most violent. But it must be remembered, that if the disproportion of the charges is very great, the action of the smaller quantity, from causes I have formerly mentioned, may be so far diminished, that the last-mentioned advantage may not take place.

SCHOLIUM.

From these principles all the various effects, that can arise from varying the quantity of the charge, the length of the piece, or the density of the bullet, may be geometrically determined; and hence all the chimerical notions, which have long obtained, in relation to these particulars, are easily confuted. For instance, it is presumed by the greatest part of the practitioners in artillery, that there is one certain charge for every piece, which will communicate to the bullet a greater velocity than any other charge either exceeding it or falling short of it; and many trials have been instituted for the discovery of this important maximum: and some professors of note have lately pretended to assign this charge, telling us, that in heavy cannon it is about $\frac{1}{3}$ of the bullet in powder, and in smaller pieces about one half of the weight of the bullet in powder. But geometers well know, that in the foregoing

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foregoing principles every augmentation of powder will increase the velocity of the bullet, unless the charge be so great as to fill up about $\frac{1}{3}$ of the cylinder; then indeed an addition to the charge may diminish the velocity of the bullet. But the quantity of powder thus assigned will in the shortest cannon amount to more, than is ever allotted for any operation of gunnery: for in a 24 pounder it will be considerably more than 30 pound.

Again, practitioners have generally supposed, that in each species of cannon there was a certain length, which communicated to the bullet fired from it a greater velocity, than could be communicated by either a shorter or longer piece: but from the propositions inserted above it appears, that this is a most groundless prejudice; notwithstanding the numerous trials, which have been instituted for its confirmation and investigation. For, by attentively considering the preceding 3d proposition, it appears, that if two pieces of the same bore, but of different lengths, are charged with the same quantity of powder; the longer piece will, rigidly speaking, communicate the greater velocity to the bullet. However, unless their lengths are extremely disproportioned, the velocities of their respective bullets will differ but little: for instance, if a musket barrel of common length and bore, be fired with a leaden bullet, and half its weight in powder, and if the same barrel be afterwards shortened by one half, and be again fired with the same charge; the velocity of the bullet in this shortened barrel will be about $\frac{1}{6}$ part less than, what it was, when the barrel was entire; and if, instead of shortening the barrel, it be increased to twice its customary length (when it will be near eight feet long) the velocity of the bullet will not be hereby augmented by more than $\frac{1}{8}$ part. And the greater the length of the piece is in proportion to the diameter of the bullet, and the smaller

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smaller the quantity of powder, the more inconsiderable will these alterations of velocity prove. So that increasing or diminishing a twenty-four pounder, for instance, by a foot in length, with its customary charge of powder, occasions no greater change than that of $\frac{1}{46}$ part in its velocity; which is a variation much too minute to be ascertained by any of the trials, which have hitherto been proposed and followed for that purpose.

From the above-mentioned principles too, it follows, that the actual velocities, with which bullets are impelled from their respective pieces, are hence easily to be assigned; whence their potential randoms, or their ranges at 45° in a vacuum, may be readily known. The geometrical process for this purpose hath been explained in another place, but is rather too complicated for this paper; and therefore to enable those, who may desire the examination of experiments of this kind, to make the necessary computations; I shall here lay down some practical rules for that end, without entering into their demonstrations.

RULE I.

If an iron bullet be fired with its weight of powder from a piece sixty diameters in length, its potential random, or its range in a vacuum, will be nearly 60000 yards; and if the length of the piece and the quantity of powder be both of them increased or diminished, its potential random will be increased or diminished in that proportion. Thus with half the weight of the bullet in powder, and piece of thirty diameters long, the potential random will be 30000 yards, and with a fourth of the weight of powder, and a piece of 15 diameters long, its potential random will be 15000 yards.

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RULE II.

If a leaden bullet be fired with its weight of powder from a piece of 90 diameters long, its potential random will be 60000 yards; and if both the powder and the length of the piece are together increased or diminished in any proportion, the potential random will be increased or diminished in the same proportion.

RULE III.

And generally, if bullets of any specific gravity whatever are fired with their weight in powder from pieces, whose lengths bear the same proportion to 60 diameters, as the gravity of these bullets bears to iron ones of the same bulk, then the potential random of these bullets will be likewise 60000 yards; and increasing or diminishing the powder and the length of the pieces together in any ratio, the potential random will be increased or diminished in the same ratio. But it must be observed, that in all these instances the bullet is supposed to lie close to the powder, without any void between, and the windage to be the least possible.

RULE IV.

To find the potential random to any given piece, bullet, and charge, proceed thus: first find what length of piece (estimated in diameters of the bullet) and what potential random corresponds in the preceding rules to the given charge of powder, call this potential random A; then if the length assigned by the rule is the same with the length of the given piece, this number A is obviously the potential random sought. But if the length of the given piece be more than the length deduced from the

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the rules; then take the difference of the tabular logarithms of these two lengths, and multiplying this difference into A (the logarithm of 10 being supposed unity) and then adding the resulting product to A, the sum will be the potential random required. If the given piece falls short of the length deduced from the rule, instead of exceeding it; then the product of the difference of the logarithms into A must be taken from A, and the remainder will be the potential random sought.

Of this process take the following example, adapted to an experiment made by *Anderson*. Suppose it be required to determine the potential random of a piece 17 inches long, carrying a five pound iron shot, and fired with three ounces of powder.

The powder here is about $\frac{1}{17}$ of the iron bullet, and the length of the piece about five diameters of the same; and by the second rule it appears, that a piece about $2\frac{1}{4}$ diameters long, charged with this proportion of powder, would have a potential random of about 2220 yards: but the piece in question being five diameters long, the difference of the logarithms of 5 and $2\frac{1}{4}$ must be multiplied into 2220, and the product, which is about 780, must be added to 2220; because the given piece is longer than the length deduced from the rule; and their sum, or 3000, is the potential random sought. Where note, that in these computations a scrupulous nicety is altogether unnecessary; a rigorous and geometrical determination being scarcely compatible with the nature of this subject.

RULE V.

The former rules being suited to those cases, where the bullet is placed home upon the powder, as in cannon; to adapt them to mortars, when a

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part of their chamber only is filled, we must proceed thus. First find, by the preceding rules, what will be the potential random, if the whole chamber was filled with powder; and then reduce the random thus found in the proportion of the whole chamber, to the part of it occupied by the given charge.

SCHOLIUM.

These are all the necessary rules for computing the potential randoms of shells and bullets; it is true, that the density of powder, and other casual circumstances, will occasion considerable varieties; and will oftentimes render the result different from these computations; but the irregularities of this kind, are neither so great, nor so frequent, as may be imagined. And those, who consider the almost infinite complication of this subject, and the ignorance in which the world has long continued of the force and mode of action of fired gunpowder, will doubtless grant, that any pretence of exhibiting the velocities communicated to bullets by its explosion, would some time since have been treated as a chimerical undertaking; and therefore I cannot but conceive, that the strange and scarcely credible coincidence of the computations here given, with the result of numerous trials made under diversified circumstances, is a matter well meriting attention, and strongly confirming the theory here advanced; especially as the causes, which often render these computations less accurate, are such, as might be reasonably expected from the nature of the theory to intervene. The circumstances, in which the trials and the computations are frequently found to differ, being those that follow.

First, If the quantity of powder be extremly small in proportion to the size of the piece, and to the weight of the bullet to be impelled; the velocity

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city communicated to the bullet will fall short of the computation. Thus, for instance, a musket-bullet, fired from a common piece with $\frac{1}{14}$ part of its weight in powder, has a velocity near a third part less, than it ought to have by the former rules.

Second, If in mortars charges are made use of, filling up but a very small part of the chamber, in this case the real velocity of the shell will fall considerably short of its computed quantity. As for instance, if into a chamber capable of holding 6lb. of powder, there is put no more than $\frac{1}{4}$lb. the velocity may not perhaps be more than half, or even a third, of what it ought to be with a comparison of the effect of larger charges.

Third, And in every case a considerable windage will sensibly diminish the effect of the powder; and, on the contrary, if the touch-hole is small, and the bullet and piece fit very exactly, the effect of the powder will be thereby augmented, especially in large charges. So that the potential randoms computed above may be then found perhaps defective, and may possibly require to be augmented a fifth or sixth part.

Fourth, There is besides some difference in the manner of placing the powder; for the same quantity of powder acts rather more violently, when it fills up a long cylinder, than when it fills a shorter cylinder, with a larger base; at least there seems to be some advantage in lengthening the chamber, till its length is near three times its diameter. In small quantities of powder this difference in the form of a chamber occasions very considerable varieties of the impulse of the bullet, as hath been formerly shewn to the society by Mr. *Hauksbee*. But in large quantities of powder, and such as bear a great proportion to the weight of the bullet, I conceive the effect here mentioned to be of less moment.

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The rules and methods of computation inserted above, assign only the potential random of the projectile in yards; but if the velocity, with which the projectile moves, be required, this may thence be easily found from the common theory of falling bodies. However, the shortest practical method of determining the velocity from the potential random, is this. From the number expressing the yards of the potential random take away $\frac{1}{11}$ of itself; then annexing two cyphers to the end of the remainder, and extracting the square root, the resulting number will be the feet, which the projectile will move in a second: not differing from the truth by much more than $\frac{1}{1000}$ part. And if the velocity was given, and the potential random was sought; this may be determined from the same principle: for multiplying the feet the body moves in a second by itself, and adding to its $\frac{1}{217}$ part, this sum, when the two last figures are cut off, will express the corresponding potential random in yards.

And now having delivered the essential rules for computing the potential random, and the original velocity of shells or bullets fired from given pieces with given charges of powder; and having in a former paper given the method of computing the actual ranges of these bodies; I have hereby completed all the precepts, which are necessary for comparing the result of any experiment with the theory.

And as an example of this comparison, I propose in the next paper to give a detail of a great variety of experiments, made in various parts of *Europe*, together with computations from the theory adapted to them; whence those, who entertain any doubts about the truth and sufficiency of the doctrine, I have hitherto advanced, may be furnished with ample materials for the discussion of their doubts, and may thence, I hope, be fully convinced,

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convinced, that the coincidence between these diversified trials and the preceding rules, can only arise from the reality and certainty of the principles, on which these rules are founded.

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No. V.

A Comparison of the Experimental Ranges of Cannon and Mortars with the Theory contained in the preceding Papers.

AFTER laying down in the preceding papers, No. II. and IV. the principles for calculating both the velocity a bullet acquires from the explosion of the powder, and the distance it ranges to, at a given angle, in consequence of the air's resistance; I shall now compare the result of these computations with the actual motions of military projectiles. And in this comparison, I hope, there will appear such a coincidence of theory with experiment, as cannot be supposed to take place in any false or fallacious hypothesis. Especially too, as all the experiments, I shall here produce, are such, as have been made by others; many of them long since printed, and all of them originally instituted with views very different from the subject of this essay. So that it cannot be pretended, I have wrested the result of them to make them suit the better with a favourite prepossession.

As it may perhaps be suspected, that the strange coincidence between the trials and our theory, which on examination frequently takes place in this subject, is principally owing to the errors in the different parts of the theory balancing each other, so that, for instance, the force of powder may be assigned too little, and the resistance of the air too great, or *vice versa:* to obviate this suspicion, I shall first establish the truth of my determination of the force of fired powder, by examining the range of shells projected from large mortars. For in these the resistance of the air is

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is on all accounts of much less efficacy than in cannon-shot. So that here a considerable change in the law or quantity of the resistance would occasion but slight alterations in the ranges; and therefore, if the actual experimented ranges of these bodies correspond to the quantities assigned by our rules, we may rest satisfied, that the real velocity, they acquired from the blast, is not very different from what we have ascribed to them; and consequently, our theory of the action and force of fired gunpowder may be safely applied to swifter and minuter projectiles.

Our first example shall be of the 18 inch sea-mortar, containing in its chamber 30lb. of powder; these mortars are generally in length about two diameters of the bore, independent of the capacity of the chamber, so that the whole must be esteemed more than $2\frac{1}{2}$ diameters. By an extract from the books of practice kept at *Woolwich* I learn, that this mortar elevated to 45°, and fired with a full charge of powder, throws a shell of $12\frac{3}{4}$ inches diameter to the distance of 3350 yards, the shell weighing 231lb.

This shell is nearly $\frac{4}{5}$ of the weight of a solid iron bullet of the same diameter, and the powder is $\frac{30}{231}$ of the shell; by computing on the principles laid down in rule the 5th of the preceding paper, the potential random will come out very near 5000 yards, and the F will be 3060 yards. But as this shell in its flight rises to the perpendicular height of about 1000 yards, where the air is rarer by near $\frac{1}{8}$ part than below. On this account the medium resistance will be diminished by about $\frac{1}{16}$ part, and augmenting the F in the same proportion, it becomes 3264, whence, by the corollary of the 2d proposition of our 2d paper, the actual range comes out 3230, differing from the experiment by no more than 120 yards; so that with this prodigious charge of powder, by far the

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greatest used in any military projectile; the actual velocity of the shell differs but inconsiderably from what is ascribed to it by the preceding theory.

The same mortar fired with 26lb. of powder, and elevated to 45°, threw its shell to the distance of 3100 yards.

Here the potential random by our rules is about 4330 yards, and, retaining the F of the last example, the actual range by our computation comes out 2980 yards, which is short of the experiment by 170 yards only.

At the same time, that I procured an account of the experiments from *Woolwich*, I learned, that a ten inch ship-mortar elevated to 45°, and fired with a full charge of powder, which was 12lb. threw its shell of 96lb. weight, to the distance of 3350 yards.

This mortar was above a diameter longer than the thirteen inch mortar of the last example, so that it cannot be esteemed shorter than 3,6 diameters; and the shell is about $\frac{24}{25}$ of a solid bullet; whence, computing on the foregoing principles, we shall find, that the potential random assigned by our theory, is about 6230 yards, and the F 2057 yards, which, properly augmented for the diminished density of the atmosphere in the upper part of its flight, becomes 2200 yards nearly, and thence the corresponding actual range should be 3190 yards, which is no more than 160 yards short of the experiment.

The next example shall be of a thirteen inch mortar for land service, being two diameters in length, its chamber capable of containing above 9lb. of powder, and its shell weighing usually about 200lb. This mortar elevated to 45°, and charged with 9lb. 1 oz. of powder, threw its shell to 2000 yards distance.

Computing upon the former principles, we shall find the potential random of this shell to be about 2727

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2727 yards, and the F to be 2672 yards; whence the actual range assigned by our rules is 1980 yards, only 20 yards different from the experiment.

The same mortar elevated still to 45°, and charged with 4lb. of powder, threw its shell to 1000 yards distance. In this instance the potential random is about 1200 yards, and the actual range thence comes out somewhat more than 1050 yards. So that here too the experiment and the computation differ but little from each other.

I have in this manner examined the ranges of mortars of all diameters, and I find, that unless the charge bears a very small proportion to the weight of the shell, or to the capacity of the chamber, the computations and the experiments correspond to a sufficient degree of exactness; and even in small charges, when proper care is taken to fit the bullet or shell to the piece, to prevent the exhalation of the flame, the actual ranges in most cases differ but very little from the computation.

From the coincidence of these diversified trials with our computation, we may safely conclude, that the force and mode of action of fired powder, is nearly such as our theory supposes; and that therefore this theory may be safely adhered to, in determining the potential randoms of every kind of artillery. For, in the examples we have exhibited, the resistance of the air is of much less efficacy than in cannon-shot: so that we cannot err much in deducing the real motions of these shells from their experimented ranges. Having thus therefore, as I presume, established that part of the theory, which relates to the action of powder; we will now proceed to the discussion of such experiments, as are best fitted to confirm the other part relating to the resistance of the air. This will be best done by the ranges of cannon-shot; the examination of which therefore shall be the business of the remaining part of this essay.

I shall

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I shall begin with those recited by St. *Remy*, in his memoirs of artillery, Vol. I. page 69; where he tells us, that pieces of cannon of the customary calibres, all of them ten feet in length, pointed at 45° and loaded with two thirds of the weight of the bullet in powder, ranged thus.

	lb.		toises.
The piece of	24	—— ——	2250
of	16	—— —	2020
of	12	—— ——	1870
of	8	— ——	1660
of	4	—— ——	1520

according to the given length of these pieces, and the diameters of the respective bullets mentioned by St. *Remy*, the potential random in *English* yards, and the F corresponding to each bullet in *French* toises, are nearly these.

	lb.	Potential Random	F to each
Pieces of	24	30000 yards	825 toises
	16	32400	720
	12	34000	650
	8	36650	570
	4	40000	450

But before we hence compute the actual ranges from our theory, it is necessary to observe, that these shot at the highest part of their flight move in air having little more than $\frac{3}{4}$ of the density of the lower part of the atmosphere; and therefore the F above assigned to each of these bullets, ought to be increased by $\frac{1}{8}$ part. When this is done, and the potential randoms are reduced on account of the treble resistance, in the manner formerly explained; we shall find the actual ranges deduced from our theory, to differ but inconsiderably from the experimented ranges given us by St. *Remy*. This will best appear by the inspection of the subjoined table.

Pieces

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Pieces. Pounders.	Experimented ranges. Toises.	Actual ranges. by our theory. Toises.	Errors of Theory. Toises.
24	2250	2255	+ 5
16	2020	2100	+ 80
12	1870	1990	+ 120
8	1660	1804	+ 144
4	1520	1558	+ 38

St. *Remy* in the same place tells us, that pieces of the same bore with those already mentioned, but somewhat shorter, were tried with half the former charge, or only $\frac{1}{3}$ of the weight of the bullet in powder, and that at 45° of elevation they ranged nearly at the same distance as the others. How well these experiments correspond to our theory, let the annexed numbers declare.

Short pieces fired with $\frac{1}{3}$ of the powder only.	Experimented ranges. Toises.	Actual ranges by our theory. Toises.	Errors of theory. Toises.
24	2250	2165	— 85
16	2020	2020	0
12	1870	1920	+ 50
8	1660	1740	+ 80
4	1520	1510	— 10

Our theory is still farther confirmed by the trials made by the *French* at *La Fere* in the year 1739, and at *Metz* in the year 1740. As manuscript accounts of both these sets of experiments have casually fallen into my hands, I shall here relate the principal experiments occuring therein.

The trials at *La Fere* were made with the usual pieces of all the preceding calibres, charged with various quantities of powder, and elevated to 4°, to 15°, and to 45°. The 24 pounder was besides fired at eight different elevations from 4° to 45°. At *Metz* no other piece but a 24 pounder was made use of; this was constantly elevated to 4°, and was loaded successively with different charges of powder from 8lb. to 20lb.

As the trials at *La Fere*, made at 45° of elevation,

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tion, do not materially differ from those recited by St. *Remy*, they require not a new examination. We shall therefore proceed to the experiments with the 24 pounder at seven different elevations short of 45°. In these the charge was always 9lb. of powder; the actual range at each time, and the corresponding numbers assigned by our theory, may be seen in the following table.

Angle of elevation.	Actual range of 24 pounder at *La Fere*. Toises.	Ditto by our theory. Toises.	Errors of theory. Toises.
40°	2050	2096	+ 46
35	2020	2080	+ 60
30	1910	2032	+ 122
25	1825	1930	+ 105
20	1740	1820	+ 80
15	1675	1620	— 55
4	820	725	— 95

In these computations I have made no allowance for the ascent of the bullet into a rarer air, which would still enlarge the computed ranges, and increase the errors of the theory. And though even then the agreement between our numbers and the actual ranges is much greater, than might be expected, considering the variety of these trials, and the uncertainty and irregularity, which constantly intervene in all experiments of this nature; yet I can venture to predict, that, if these trials are repeated with due care, and necessary precautions are taken, the difference between their result and our numbers will be still less, than what appears in the foregoing comparison; there being several reasons, not necessary to be here discussed, which render all, that was done at that place, less proper for the examination of a theory, than what was afterwards executed at *Metz*. But to proceed.

The next trials in order were those made at *La Fere* with other pieces at different angles. All these,

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these, with the corresponding numbers deduced from our theory, I have comprised in the annexed table: where I must observe, that the same angle, which in some places of the manuscript is denominated 4°, in other places is presumed to be 5°. And if this be allowed, our numbers, which in these small angles at present are defective, will then agree to the trials without sensible error.

Pieces.	Pounds of powder.	Angle elevation.	Actual range. Toises.	Actual range by theory. Toises.	Errors of theory. Toises.
16	6	15	1780	1540	240
16	6	4	825	707	118
12	4	15	1500	1440	60
12	4	4	820	680	140
8	3	15	1440	1320	120
8	3	4	770	650	120
4	2	15	1500	1200	300
4	2	6	862	780	82

Having now done with the experiments made at *La Fere* in 1739, where from the nature and intention of the trials, and the exceptionable manner of selecting them, a nearer coincidence with a true theory could not be expected: we will next proceed to the comparison of those made at *Metz* the year following; these were executed with much more care and attention, and are related with more candour and distinctness; for here the result of each trial is set down; whereas in the others the mediums only were given, and these too curtailed according to the fancy of the relator. The only piece made use of at *Metz* was a 24 pounder 10 feet in length, as I have already observed; this was loaded with different charges of powder from 8lb. to 20lb. It was constantly elevated to 4°; but as it was placed 78 feet above the plain, on which the bullets fell, this amounts at a medium to another degree of elevation; esteeming

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ing therefore the angle to be 5°, the result of all these trials (which were continued for three days) is here represented, together with the corresponding ranges deduced from our theory.

Pounds of powder.	Actual range. Toises.	Ditto by our theory. Toises.	Errors of theory. Toises.
8	799	836	+ 37
—	844	—	— 8
—	829	—	— 7
—	887	—	— 51
9	715	843	+ 128
—	917	—	— 74
—	855	—	— 12
—	812	—	+ 31
—	742	—	+ 101
—	806	—	+ 37
—	870	—	— 27
—	854	—	— 11
—	854	—	— 11
—	822	—	+ 21
—	858	—	— 15
—	826	—	+ 17
—	808	—	+ 35
—	856	—	— 13
—	1010	—	— 167
—	735	—	+ 108
—	900	—	— 57
—	783	—	+ 60
10	834	850	+ 16
—	872	—	— 22
—	851	—	— 01
—	845	—	+ 05
—	871	—	— 21
—	838	—	+ 12
11	837	857	+ 20
—	784	—	+ 73
—	950	—	— 93
—	892	—	— 35

Pounds

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Pounds of powder.	Actual range. Toises.	Ditto by our theory. Toises.	Errors of theory. Toises.
—	792	—	+ 65
—	830	—	+ 27
12	812	864	+ 52
—	807	—	+ 57
—	882	—	— 18
—	899	—	— 35
—	842	—	+ 22
14	840	878	+ 38
—	848	—	+ 30
—	878	—	— 0
—	950	—	— 72
—	1060	—	— 182
—	843	—	+ 35
16	1000	891	— 109
—	898	—	— 07
—	970	—	+ 79
—	835	—	+ 56
18	950	903	— 47
—	1000	—	— 97
20	1100	916	— 184
—	841	—	+ 75

From the inspection of these numbers we may collect the strongest proof of what we have frequently asserted upon other occasions; "That "our theory differs less from the experiments, "than the experiments do from each other." Since here, in the repeated trials with nine different charges of powder, the least of 8lb. and the greatest of 20lb. there is but one charge only, and that repeated but twice, where the differences from the theory fall the same way. I therefore cannot doubt, but if a like series of trials with these were made at much greater angles, the result would be altogether as consonant to our numbers, though it is evident enough from the preceding table, that little regard can be paid to single trials; since in the

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the two experiments with 20lb. of powder, the difference in range is 259 toises, and with 9lb. of powder the difference of two subsequent ranges is 275 toises; whereas the theory in no instance differs from the medium of any of these trials more than 75 toises. Hence I conclude, that till a number of trials shall be made at large angles; it cannot be determined, whether the great differences, which occur in the examination of some of the ranges at *La Fere*, are to be imputed to the theory, or to the uncertainty and insufficiency of the trials themselves.

The trials hitherto considered might abundantly suffice for the confirmation of our principles; but before I conclude this subject, I chuse, for particular reasons, to examine the experiments made at *Woolwich* in the year 1786 with 24 pounders of different lengths. These experiments were undertaken on a false presumption, that there was a certain length for cannon, which enabled the piece to shoot more efficaciously, than it would do, if it were either longer or shorter. To determine this proper length, pursuant to this erroneous hypothesis; six 24 pounders were cast of the same weight, the shortest eight feet long, and the longest ten feet and a half. These were all loaded alike, and were elevated to $7°\frac{1}{4}$, at which elevation five shot were made with each, and the mediums taken; whence an estimate was proposed to be made of the effects of these different lengths, and it was from thence decided, that the pieces of 9 and $9\frac{1}{2}$ feet long, ranged farthest.

Before we compare the result of these ranges with our theory; it will not be impertinent to confute the fallacious determinations, which have been from thence deduced. By our theory the longer pieces ought to range farthest; but the varieties are too minute to be ascertained by trials of this kind. Since on our principles the ranges with the

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pieces 9 and $9\frac{1}{2}$ feet long ought not to differ by more than 35 yards from the ranges of the shortest and the longest. And yet two subsequent trials with the 9 feet piece differ from each other 650 yards; and the medium of the trials with that piece on different days (for they were repeated three days successively) differ from each other 300 yards. It appears then from hence, that it will be a vain attempt to endeavour to ascertain the effects of cannon of unequal lengths by trials of this nature, which on a repetition vary twenty times more from each other than the whole difference by this means proposed to be discovered. But to proceed.

If in these experiments the powder was half the weight of the bullet (which, I conceive, was nearly the case) the potential random then to pieces of 9 and $9\frac{1}{2}$ feet long, which are the mean lengths, will be by the theory about 25,000 yards, which reduced on account of the treble resistance becomes about 16,200 yards, and the diameter of a 24 pound shot being nearly 5 and $\frac{2}{3}$ inches, F is 1700 yards, whence the actual range at $7^{\circ}\frac{1}{4}$ is about 2400 yards. And as these shot all fell into the water, if the surface of the water be supposed 10 yards lower than the platform, on which the pieces were placed, the range will be augmented at a medium near 60 yards more on that account; whence the whole becomes 2460 yards, which is sufficiently consonant to the trials; for I find, that a medium of all the shot made with the different pieces was on one day 2584, and on another day 2524, and on the third day 2470, where the greatest difference is on the first day, and amounts to 124 yards; which, after what has been observed of the irregularity of these trials, must appear an error too slight to be regarded.

And here I might safely finish this examination; but as *Eldred*, our countryman, who, above

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a hundred years since, was master-gunner of *Dover* castle, hath delivered in his writings some trials made in very unusual pieces with uncommon charges of powder; I shall conclude with inserting a few of his experiments, both to shew their conformity with our theory, and as a matter of curiosity likewise.

The first experiments, I shall relate from him, were those made with a demi-culverin 10 feet 8 inches long, carrying a 9 pound ball, and loaded with 7lb. of powder.

This piece elevated to 10°, and having 2° more on account of the advantage from the height of the clift, upon which it stood, ranged to the distance of about 2840 yards.

If the powder used by *Eldred* was of the same fabric with the present government powder, the potential random would be about 38400 yards, which reduced for the treble resistance becomes 18600. And F being 1200 yards, the actual range at 12° is by the theory 2820 yards, agreeing with great nicety to the experiment.

But the most singular trials, related by *Eldred*, are those, he made with what, he styles, the basilisk. This is the long piece well known to all, who have seen *Dover* castle. *Eldred* tells us, that this piece, which carries a 10lb. shot, and is 23 feet in length, was tried by him several times with a load of 18lb. of powder, and that at two degrees of elevation, it ranged 1200 yards, and at 4° $\frac{3}{4}$ it ranged little short of 2000.

This piece may be esteemed about 65 diameters in length, and according to our theory the potential random with 18lb. of powder should be about 84000 yards, which reduced on account of the treble resistance becomes 24200 yards, and the F to a 10lb. shot being 1240 yards, and the actual range at 4° $\frac{3}{4}$ is by computation 2080 yards, and the actual range at 2° about 1180 yards, both of

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them extremely near *Eldred's* experiments; only it must be observed, that in very small angles, and very large potential randoms, the method of reduction we here, follow on account of the treble resistance, will give the actual ranges somewhat less than their true quantity.

I must observe, that these last experiments, with the basilisk, furnish an incontestable proof of the prodigious augmentation of the resistance in great velocities beyond its customary rate; since from other experiments I am satisfied, that this piece, with 2lb. of powder only, or $\frac{1}{9}$ of the charge given by *Eldred*, would have ranged at $4^{\circ}\ \frac{3}{4}$ to full 1500 yards, unless its windage is enormous. So that by augmenting the charge ninefold, the actual range is only increased in the proportion of 3 to 4. Nor let it be supposed, that in this great charge the rules for computing the velocity of the bullet, or its potential random, may prove defective, on account of the immense celerity, with which the flame of the powder must expand to continue its impulse all through the cylinder; let not this, I say, be supposed, since by experiments, which I myself have formerly made, and which are recited in another place; it appears, that in a barrel similar to the basilisk, but of a small bore, a charge of powder, which filled up $\frac{1}{6}$ of the piece, and therefore in proportion to the piece was greater than that used by *Eldred*, was not found in its effect to be deficient from theory, but rather exceeded it, as indeed all large charges, instead of falling short of the force to be expected from them in proportion to their quantity (which according to the vulgar prepossessions on this head ought constantly to happen) do never fail to receive some additional efficacy from their increased quantity, beyond what is assigned by theory; so that I do not conceive, the potential random, assigned above to the shot made with the basilisk,

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was more than it actually received, unless the powder was inferior to that used at present, which, I know not that there are any reasons to believe. If then the potential random of these shot made by *Eldred* is nearly of the quantity exhibited by our computation, or about 84000 yards or 47 miles; it follows, that its actual range at 4° $\frac{1}{4}$, as tried by *Eldred*, is not the seventh part of the potential range at the same angle. And its actual range at 45°, I affirm, will be little more than the twentieth part of its potential random, or its range at the same angle in a vacuum; so prodigious are the effects of the air's resistance on these ponderous bodies.

These are all the experiments I have thought necessary to insert in the present essay; not that they are a tenth part of those, I have collected and computed for my own satisfaction. I have chosen experiments made by others, rather than such as have been made by myself, that no suspicion might arise of their being unfairly twisted to a concurrence with my theory. And I have selected such, as were made with large mortars and cannon; because it hath been urged against the experiments formerly recited in my new principles of Gunnery, that as they were made in very small pieces, their coincidence with the theory was no proof, that in large quantities of powder the same would take place.

*** *The following tracts also have been never yet published, except the second, which is now reprinted from a copy corrected by the author.*

Practical

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Practical Maxims relating to the Effects and Management of Artillery, and the Flight of Shells and Shot.

THE preceding papers, containing the general principles of the real motions of projectiles; it might perhaps be imagined, that after the many proofs already given of the coincidence of these priciples with the actual motions of shot and shells of different kinds, nothing more need be added, in explanation or confirmation of this new theory. But as it frequently happens, that the clearest deductions are of little efficacy against the influence of long established prejudices; I therefore thought it might not be altogether useless to trace some of the articles relating to this subject in a more popular manner; as I conceived, they would be more readily considered in that form, by those, whose prepossessions would prevent them from a too laborious attention. And, as this essay is principally intended to rectify the erroneous opinions, which at present prevail in relation to the nature and effects of artillery; I thought that purpose would be most effectually answered by distinctly discussing those opinions and evincing their falsehood, and the fallacy of the experiments on which some of them are pretended to be founded. For though the establishing the true principles of any art by incontestable proofs may be thought a sufficient confutation of all such suppositions, as are contrary thereto; yet with such, as are biassed by authority, prescription, and habit, this general argument is of little moment; it being necessary for their conviction (if that be at any time possible) to enter into a formal examination of their favourite hypothesis, whatever it be, and to shew

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where and in what manner their conclusions are defective. This being premised, we now proceed to our maxims.

MAXIM I.

In any piece of artillery whatever, the greater quantity of powder it is charged with, the greater will be the velocity of the bullet.

It is vulgarly supposed, that the powder, with which any piece is loaded, fires gradually all the time, the bullet is passing through the barrel; and thence it has been concluded, that there was a certain charge, which would be just consumed at the parting of the bullet from the mouth of the piece. This, it was determined, was the proper charge for the piece in question; and if more powder than this was made use of, it was presumed, that the added part would not take fire, and therefore would by its weight diminish the action of the rest; so that from this reasoning it followed, that by augmenting the powder beyond the supposed proper charge, the velocity of the bullet would be diminished. And it is usually imagined by the modern artillerists, that this proper charge is not more than one half, nor less than one third, of the weight of the bullet in powder.

This is the substance of the usual speculations on the present subject; every part of which is altogether erroneous. For neither does powder fire in the gradual manner here supposed (as we have amply demonstrated in another place) nor is there any practical charge so great, but that, if it be augmented, the bullet will be thereby more vehemently impelled. For, examining a piece of the smallest bore in general use, and which was about 30 diameters in length, I found, that when with an iron ball and one half of its weight in powder it was

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was fired against a beam of oak placed ten yards from the muzzle, the bullet penetrated at a medium to the depth of about five inches; but when fired with three times that charge, or once and a half the weight of the bullet in powder, the penetration into the same beam was not less than 10 inches. I have made many trials of this kind (for in small pieces there is little difficulty in repeating them) and I have never failed to observe, that increasing the charge, at least as far as to twice the weight of the bullet, always augmented the penetration of the ball into a solid body; whence its augmented velocity is easily evinced. And if it is asserted, that in larger pieces, whose lengths are less in proportion to their bores, the same effects would not take place: I should answer, that if the method of proving a 24 pounder (the largest piece in common use) be attended to; the circumstances occurring therein will be a sufficient confutation of this assertion. For if the heat of the piece, the violence of the explosion, and the penetration of the bullet into the butt of earth be examined; it will presently appear, that with the whole weight of the bullet in powder, or even two thirds of that weight (the usual quantities for proof) the velocity of the shot is much greater than with the customary charges, which are about half these quantities. Since if (as is usual) the butt of earth for receiving the balls be but little removed from the mouth of the piece; it will be found, that the depth, to which the bullet pierces, with the proving charge, will be more than a third part greater than with the charges commonly allotted for service. Hence then we may conclude, that the supposition, that an augmentation of powder beyond the usual quantity diminishes the effort of the bullet, is in every instance erroneous.

If it be demanded, how an opinion, which is to be thus easily confuted by facts, which occur in

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almost every day's practice, could be so long and so eagerly supported; I answer, that the plausible hypothesis of the gradual firing of the powder did doubtless first give rise to it. And when it was once advanced as a matter of speculation, there were not wanting those, who pretended to confirm it by experiments on the ranges of pieces differently charged. What these experiments were, and wherein their fallacy consisted, we shall explain more at large hereafter.

I have, in the establishing this maxim, confined myself to the consideration of the charges and pieces in general use. For if a piece be so short, or the charge be so great, that the powder takes up about $\frac{3}{5}$ of the whole cavity of the cylinder; then indeed an augmentation of powder will not increase the celerity of the bullet, as I have determined elsewhere upon principles too complicated to be here explained. But this is a case, which can never occur in practice; and therefore the consideration of it may be safely neglected in discussing the mistaken opinions of practitioners.

MAXIM II.

If two pieces of the small bore, but of different lengths, are fired with the same charge of powder, the longer will impel the bullet with a greater celerity than the shorter.

The supposed gradual firing of the charge, discussed above, hath been also applied to the consideration of the length of pieces; whence it hath been usually concluded, that there is a certain length, which, if a piece exceeds, the velocity of the shot will be thence diminished. And some practitioners have determined this length for a 24 pounder, when fired with half the weight of the bullet in powder,

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powder, to be about twenty times the diameter of its bore.

As some of the extraordinary culverins, cast many years since, and about 60 diameters in length, and the shortest cannon in general use contain no more than 20 diameters; I have therefore examined the penetration of an iron ball into a block of wood, when fired from a small piece, which was first 60 diameters long, and was afterwards reduced to 20 by cutting. The charge was each time half the weight of the bullet in powder, and I found, that with 60 diameters the penetration at a medium was above half as much again as with 20 diameters. And I dare affirm, that whoever shall make these experiments with pieces of any bore whatever, will find the result not much different; provided due care be taken, that the powder be in good condition, that the substance into which the ball penetrates be placed so near the piece, that the resistance of the air may occasion but little alteration, and that the substance itself be of a uniform texture.

Since then the old culverins, which are more than 20 feet long, and which from their unsizeable and unmanageable length have been long since laid aside, do yet impel their bullet with more violence than any piece of a shorter fabric; it may be safely concluded, that within the limits of practice there is no piece so long, but if its length be augmented, an addition will thereby be made to the impetuosity of its shot; and consequently our maxim may be safely adhered to in all practical discussions on this subject. Indeed, if the force of the powder, either by exhaling or expanding, be so far diminished, that it becomes less than the resistance and friction of the bullet in the piece; then the piece by being shortened will shoot with more efficacy; but this is a case, that with the customary charges cannot happen, except the lengths

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lengths greatly exceed any ever yet tried. And therefore the consideration of this case would be altogether superfluous in settling the maxims, by which the practice of artillery ought to be regulated.

MAXIM III.

If two pieces of artillery different in weight, and formed of different metals, have yet their cylinders of equal bores and equal lengths; then with like charges of powder and like bullets they will each of them discharge their shot with nearly the same degree of celerity.

For as these pieces must be supposed of sufficient substance to resist the effort of the customary charges of powder without sensibly changing their figure; no good reason can be given, why they should differ materially in their effects. Since as to their momentary extension during the explosion, and the elasticity with which they again restore themselves, though these may be different in different metals; yet the whole of this inequality is so small, that the variety arising from thence must be altogether insensible. And though the different weights of the pieces is a circumstance, which in rigour will occasion an assignable inequality in the motion of the shot; yet, as the celerity of the bullet, from the lightest cannon ever made use of, is not in similar trials defective by a hundredth part, from what it would be in the heaviest; the consideration of these niceties may be safely neglected in our present disquisition. Indeed, in the establishing of this maxim, I have not relied merely on speculation; for I have caused several pieces to be made of the same bore and length, but of different metals, and very different in

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in their weight and substance; and after a number of trials with each, I have never been able to discern, that in like circumstances there was any constant difference in the force, with which they discharged their shot; so that we cannot but insist on the truth of the present maxim (at least in all practical views) how opposite soever it may appear to many long established prepossessions.

MAXIM IV.

The ranges of pieces at a given elevation are no just measures of the velocity of the shot; for the same piece fired successively at an invariable elevation, with the powder, bullet, and every other circumstance as nearly the same as possible, will yet range to very different distances.

The varieties in the ranges of the same piece, with the same charge and elevation, are scarcely credible to those, who have not been conversant in trials of this kind. These irregularities are vulgarly ascribed to the powder; but were that the true cause, it could only produce an inequality in the extent of the range; whereas we frequently find, that, besides a different length of range, the bullet deviates greatly to the right and left of the line, in which it was discharged. I myself have seen a piece fired with great care in an invariable position; and yet two successive shot of it have flown in directions, which on the horizontal plain made an angle with each other of above fifteen degrees.

MAXIM

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MAXIM V.

The greatest part of that uncertainty in the ranges of pieces, which is described in the preceding maxim, can only arise from the resistance of the air.

For as it appears, from what hath been already observed, that the bullet, after it is separated from the piece, is frequently deflected to the right and left of its original direction; there is no other power known but the resistance of the air, to which this effect can be imputed.

MAXIM VI.

The resistance of the air acts upon projectiles in a twofold manner; for it opposes their motion, and by that means continually diminishes their celerity; and it besides perpetually diverts them from the regular course, they would otherwise follow; whence arise those deviations and inflections, of which we have been just now treating.

The reality of these assertions will be evinced in the discussion of the following maxims.

MAXIM VII.

That action of the air, by which it retards the motion of projectiles, though it has been hitherto neglected by the writers on artillery, is yet in many instances an immense force: and hence the motion of these resisted bodies is totally different, from what hath been generally supposed.

The

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The theorists, who have endeavoured to apply the science of motion to the subject of artillery, have usually premised, that the resistance of the air to shells and cannon-bullets was so small in proportion to the weight of those bodies, that their motions were not sensibly affected by it. Hence it was concluded, that the track described by military projectiles was the curve of a parabola. And hence two rules were given for assigning their ranges at any angle of elevation; provided the velocity, they were discharged with, was known. As these rules are undoubtedly true, supposing the resistance of the air to be insensible: I conceived the best method of examining, how far this supposition might be relied on, was to compare those rules with the actual ranges of cannon and mortars. And in making experiments with this view I found, that though in the ranges of shot discharged with small charges, and consequently with minute degrees of celerity, the effects of the air's resistance were not considerable; yet by augmenting the charge, and thereby increasing the velocity of the projectile, the action of the air thereon became more and more vigorous. And to cannon-shot discharged with their customary allotment of powder, it amounted to an almost incredible force. I find, for instance, that, when a 24 pound shot is impelled by its usual charge of powder, the opposition of the air is equivalent to at least 400lb. wt. which retards the motion of the bullet so powerfully, that, if it is fired at 45° elevation, its range is not a fifth part of what it would be, were the resistance of the air prevented. In lighter and smaller shot this is still more observable; for I have made many experiments with a wooden bullet fired at 45°, where, instead of 15000 yards, which it would have flown in a vacuum, it has not actually ranged to 200 yards; so that the resistance has taken away 74 parts in 75 of the whole range.

It

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It is sufficiently evident, that the swifter the projectile moves, the stronger the resistance acts against it; and on a nicer examination it will be found, that to a double velocity there wil lbe nearly four times the resistance, and to three times the velocity, nine times the resistance, and so on. But then this rule must not be extended to the comparison of the resistances of two velocities, one of which is less, and the other greater than that of 1200 feet in a second; for I find, that to velocities exceeding this last-mentioned, the resistance is three times as great, as would come out by a deduction from what takes place in slower motions. And as we shall hereafter find, that many of the extraordinary effects of artillery, which have given rise to much contestation, are the natural consequences of this sudden trebling of the resistance, we therefore think it expedient to insert the substance of what we have here observed, amongst our maxims, and have framed the two following ones for that purpose.

MAXIM VIII.

This retarding force of the air acts with different degrees of violence, according as the projectile moves with a greater or lesser velocity; and the resistances observe this law: that to a velocity, which is double another, the resistance (within certain limits) is fourfold, and to a treble velocity, ninefold, and so on.

MAXIM IX.

But this proportion between the resistances to two different velocities does not hold, if one of the velocities be less than that of 1200 in a second, and the other greater.

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For in that case the resistance to the greater velocity is near three times as much, as it would come out by a comparison with the smaller, according to the law explained in the last maxim.*

The proof of the two last maxims depends upon experiments, which I have described in another place; but which require an ampler discussion than suits with the nature of this essay.

In the same dissertation, where those experiments are considered, I have also shewn, that the resistance on a 12 pound iron shot, moving with a velocity of 25 feet in a second, is equivalent to about $\frac{1}{2}$ an ounce avoirdupois; from whence by the help of the two preceding maxims, the quantity of resistance on any shot or shell, moving with a known velocity, is easily assigned. For instance, if the 12 pound bullet moves at the rate of 100 feet in a second (that is four times 25) its resistance will be 16 times half an ounce, or half a pound. And if it moves at the rate of 1000 feet in a second, which is 10 times the last velocity, its resistance is 100 times as much, or 50 pounds. But if the velocity be that of 2000 feet in a second, or twice the last; then its resistance, instead of being only four times as much, or 200 pounds, is treble that quantity, or equivalent to 600 pounds.

If the bullet or shell be of any size whatever, the resistance thereon is easily deduced from the proportion of its surface to that of a 12lb. shot. For two shells or bullets, moving with equal velocities, are always resisted in the proportion of their surfaces.

MAXIM X.

To the extraordinary power exerted by the resistance of the air it is owing, that, when two

* These last two maxims require some considerable modification. See the note at p. 181. H,

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two pieces, of different bores are discharged at the same elevation, the piece of the larger bore usually ranges farthest, provided they are both fired with fit bullets, and the customary allotment of powder.

The matter of fact contained in this maxim cannot have escaped the notice of any one the least conversant in the practice of artillery; it being obvious even to the most incurious observer. Thus a 24 pounder loaded in the customary manner, and elevated to 8°, ranges its bullet, at a medium, to about a mile and a half; whereas a three-pounder, which is of half the diameter, will in the same circumstances range but little more than a mile. And the same holds true in other angles of elevation; though indeed the more considerable the angle of elevation, the greater is the inequality of the ranges. Now this diversity in the ranges of unequal bullets cannot be imputed to any difference in their velocities: since when loaded alike they are all of them discharged with nearly the same celerity: but it is to be altogether ascribed to the different resistances, they undergo during their flight through the air. For though the 24 pound shot, for instance, has four times the surface, and consequently four times the resistance of the 3 pound shot; yet as it has 8 times the solidity, the whole retarding force, which arises from the comparison of the resistance with the matter to be moved, will be but half as much in the 24 pounder as in the 3 pounder. And thus it will always happen (whatever be the size of the bullet) that the retarding force of the air on the lesser bullet will be greater than the retarding force on the larger in the same proportion, as the diameter of the larger bullet is greater than the diameter of the lesser. Now since, from what we have already

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already shewn, it may appear that the range of shot at an elevation is much more influenced by the quantity of its resistance, than by the velocity it is discharged with; it is not to be wondered at, that a 24 pound shot, being but half as much retarded as a 3 pounder, should range considerably farther.

MAXIM XI.

The greatest part of military projectiles will at the time of their discharge acquire a whirling motion round their axis by rubbing against the inside of their respective pieces; and this whirling motion will cause them to strike the air very differently, from what they would do, had they no other but a progressive motion. By this means it will happen, that the resistance of the air will not always be directly opposed to their flight; but will frequently act in a line oblique to their course, and will thereby force them to deviate from the regular track, they would otherwise describe. And this is the true cause of the irregularities described above in maxim IV.

That bullets must in general acquire a whirling motion on their discharge, will not, I presume, be disputed. And, that this whirl, by combining with their progressive motion, will occasion the action of the resistance to be olique to their direction, is evinced by experiment. For if any pendulous body be made to revolve about the line, to which it is suspended, (which may be effected by various methods), such a pendulum will in its vibration always deviate from the direction originally given to it. And the deviation will con-

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stantly incline to that hand, towards which the revolving motion tends. The same is visible in tennis-balls, where they are made to revolve round their axis by an oblique stroke of the racket; and in musket-shot the deflection arising from this cause is frequently sensible even in the distance of 100 yards, as I have sufficiently evinced in one of the preceding papers. Indeed in both musket and cannon-shot it is impossible to foresee to what quarter the deflection will tend; since it is impossible to predict, what will be the position of the axis, round which the bullet will turn. For from the irregularity in its friction, before it gets clear of the piece, it is not reasonable to expect, that any two shot, though discharged from the same piece, should revolve in the same manner. Not to mention that probably the axis of revolution frequently shifts its position during the flight of the shot. Hence we may upon the whole conclude, that the greatest part of shells and bullets are perpetually deviating from their regular track in consequence of their whirling motion; yet neither the tendency nor quantity of this deviation can in general be determined beforehand; nor will the nicest repetition of similar experiments produce any resemblance in the circumstances of these irregularities.

MAXIM XII.

From the sudden trebling the quantity of the air's resistance, when the projectile moves swifter than at the rate of 1200 feet in a second, (as hath been explained in maxim IX.) it follows, that whatever be the regular range of a bullet discharged with this last-mentioned velocity, that range will be but little increased, how much

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much soever the velocity of the bullet may be still farther augmented by greater charges of powder.

For by the extraordinary reinforcement of the resistance in all velocities surpassing that of 1200 feet in a second, the motion of the bullet, how swift soever it be, is soon reduced to this last-mentioned rate. I find, for instance, that a 24 pound shot, when discharged with a velocity of 2000 feet in a second, will be reduced to that of 1200 feet in a second in a flight of little more than 500 yards. And yet with the greatest of these velocities, the bullet will range farther than with the least by above fifteen miles, supposing both shot to be fired at 45°, and neither of them to be impeded by the resistance of the air. Now as the velocity of 2000 feet in a second is much greater, than what a 24lb. shot receives even from two thirds of the weight of the bullet in powder; and the velocity of 1200 feet in a second may be produced by little more than a fourth part of the charge; it appears from hence how speedily all the additional celerity is taken away, which arises from the utmost reinforcement of the charge beyond that of a fifth or a sixth of the weight of the bullet in powder. Whence as any excess in the velocity of the projectile above that of 1200 feet in a second is thus precipitately destroyed by the resistance; it is easy to conceive, that the regular range of a bullet, fired at any considerable angle with the greatest charge possible, will but little exceed the range of the same bullet, when fired with a fifth or sixth of its weight in good powder. I find, for instance, that the regular range at 10° of a 24 pounder, fired with 24lb. of powder, will not exceed the range of the same piece at the same angle, when fired with only 5lb. of powder, by more than 500 yards; the whole range being above

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3000; so that with charges in the proportion of 5 to 24 the ranges are only as 5 to 6. And this not from any defect of the action of the powder in the larger charge, but solely from the extraordinary action of the air's resistance. In smaller pieces, fired with charges in these proportions, the ranges approach yet nearer to an equality, still provided that the ranges here understood, are those which I denominate regular, or such as would be described by the bullet, supposing it uninfluenced by the action of that deflecting force, which is the subject of the eleventh maxim. What varieties this force will give rise to, we shall now proceed to consider.

MAXIM XIII.

If the same piece of cannon be successively fired at an unvariable elevation, but with various charges of powder, the greatest charge being the whole weight of the bullet in powder, and the least not less than the fifth of that weight; then if the elevation be not less than 8° or 10°, it will be found, that some of the ranges with the least charge will exceed some of those with the greatest.

For we have seen in the last maxim, that at 10° of elevation the difference between the regular ranges of a 24 pounder, with these very unequal charges, amounts to but about 500 yards. Now the ranges with the same charge, and every circumstance as near as possible the same, will at an elevation of 10°, vary sometimes 6 or 700 yards from each other, in consequence of the deflective force discussed in the eleventh maxim. And as the deflective force may casually augment the range with the

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the smallest charge, and diminish the range with the greatest charge; it is evident, that the smaller charge may in these circumstances considerably outrange the larger, notwithstanding the much greater force, with which the larger charge impels the bullet in the explosion.

MAXIM XIV.

If two pieces of cannon of the same bore, but of different lengths, are successively fired at the same elevation with the same charge of powder; then it will frequently happen, that some of the ranges with the shorter piece will exceed some of those with the longer.

For the shortest piece of cannon I have yet seen being not less than 15 diameters, and the longest scarcely exceeding 60 diameters; the difference of the velocities of two shot discharged from these pieces, with any of the customary charges of powder, will amount to but little more than a fourth of the greater velocity. And we have seen in the last maxim, that when the difference of velocity was much greater than this; yet the action of the whirling force, by prolonging one range, and contracting the other, has occasioned the slower bullet to be projected to the greater distance.

And hence it may be concluded too, that if the longer piece be allowed a greater charge of powder than the shorter; yet the same effect will follow, provided the velocities produced by those charges are within the limits described in the preceding maxims; that is, provided the charge of the shorter be not less than one fourth of the weight of the bullet, nor the charge of the longer more than the whole weight of the bullet. The truth of this principle I have frequently experienced, par-

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ticularly in a comparison of one of the short six pounders lately cast at *Woolwich*, with a six pounder of the old fabrick, of four times the weight and near twice the length. For these being both elevated to 11°, and the short piece loaded with 1lb. $\frac{1}{4}$ of powder, and the long piece with 3lb. it was found, that the fourth shot of the short piece ranged 2432 yards, whilst the first shot of the long piece ranged only 2422 yards, and the third shot from the same piece no more than 2070 yards.

MAXIM XV

In distant cannonadings the advantages arising from long pieces, and large charges of powder, are but of little moment.

The truth of this maxim may be easily deduced from the preceding observations; as it thence appears, that neither the distance to which a bullet flies, nor its force at the end of its flight, are considerably increased by very great augmentations of the velocity, with which it is impelled from the piece.

MAXIM XVI.

In firing against troops with grape shot, it will be found, that charges of powder much less than those generally used, are the most advantageous.

For grape shot fired with large charges are dispersed from their intended direction, and the greatest part of them lost; whereas with small charges they fly steadily and closely, and by that means make much greater ravage amongst the troops they are fired against. For it must be remembered with regard to the force of the bullets; that charges of powder, extremely minute, are yet sufficient to impel either round or grape shot with more violence, than is necessary for giving a mortal wound.

MAXIM

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MAXIM XVII.

The principal operations, in which large charges of powder appear to be more efficacious than small ones, are the ruining of parapets, the dismounting of batteries covered by stout merlons, or battering in breach. For in all these cases, if the object be but little removed from the piece, every increase of velocity will increase the penetration of the bullet.

This (after what has been said above) needs but little explanation. I shall only observe, that if the piece in question be 4 or 500 yards distant from the object, then a reinforced velocity will scarcely produce any sensible augmentation in the depth, to which the bullet penetrates. Or if the object be near the piece, but the parapet is so thin, as to be pierced through its substance by a bullet fired with a small charge, in that case a larger charge or a greater velocity, instead of increasing the effect, will diminish it. For the ravage occasioned in a solid body by a bullet, that passes through it, is always greatest, when the bullet just loses its motion at the last surface of the body. This is most evident in firing against butts of wood or the sides of ships; for there the bullet, which has but just force to get through, generally breaks and splinters the last surface, and drives great shivers before it; whereas if it moves with much more force, and has thereby a considerable velocity left, after it has passed through; the penetration will be generally no more than a hole, which is in great measure closed up too by the springiness of the wood.

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MAXIM XVIII.

Whatever operations are to be performed by artillery, the least charges of powder, with which they can be effected, are always to be preferred.

For all addition to the charge, beyond what is sufficient for executing the purpose in hand, is not only an unnecessary waste of powder, but is attended besides with much more important disadvantages; since hereby the piece is heated, and strained, its recoil becomes more violent, its carriage labours more, and is more liable to be disordered; whence the piece is frequently silenced. And at best its service is much slower, than it would be with a smaller charge. If, besides these inconveniences, it be farther considered, that the effect of the bullet is frequently diminished by augmenting its velocity; it should seem, that the justness of our maxim was liable but to little contestation. But to leave no room for doubt, in a precept so much opposed to the modern practice, we will discuss it still more circumstantially.

The principal employments of artillery, are the canponading of troops either at a distance or near at hand; the firing against ships; the ruining of defences, or the battering in breach. Now we have already shewn, that in distant cannonadings large charges scarcely range farther than smaller. We have also seen, that in firing against troops within the reach of grape-shot, small charges are the most efficacious. And in firing against ships, we have observed above, that the charge, which is just sufficient to impel the bullet through one side only, is more mischievous to the vessel and destructive to the crew than a larger charge, which should drive the shot through both. Hence then the ruining of defences, and the battering in breach are

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are the two only operations, where it can be pretended, that large charges are of use; but even these operations, when executed, as they frequently are, at 4 or 500 yards distance, receive but little advantages from very great augmentations of powder. And since both the parapets of all fortified places, and the masonry, which face their ramparts, are often ruined at these great distances, where the influence of the largest charges is considerably abated; it appears most adviseable to avoid the inevitable inconveniences of large charges, how near soever the battery may be to the place. For though with less powder the penetration may be somewhat diminished; yet the security and dispatch in the service of the pieces will more than compensate for that deficiency, and will be found, upon the whole, the most speedy and effectual practice.

MAXIM XIX.

Hence then the proper charge of any piece of artillery, is not that allotment of powder which will communicate the greatest velocity to the bullet (as most practitioners have hitherto maintained) nor is it to be determined by an invariable proportion of its weight to the weight of the ball; but, on the contrary, it is such a quantity of powder, as will produce the least velocity necessary for the purpose in hand; and, instead of bearing always a fixed ratio to the weight of the bullet, it must be different according to the different business, which is to be performed.

This maxim, I presume, will not be controverted by those, who have attentively considered, what hath

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hath been already advanced. But as it may be expected, I should give a more explicit determination of what I deem the proper charges of pieces, in different circumstances; I shall here dispatch that matter with as much distinctness, as I can. Observing first that the powder, of which I at any time speak, is supposed to be made by the government's standard, of good materials, and to be in good condition at the time of using. And next that the windage be moderate: otherwise some addition must be made to the charges assigned in the following maxim.

MAXIM XX.

No field-piece ought at any time to be loaded with more than $\frac{1}{6}$, or at the utmost $\frac{1}{5}$ of the weight of its bullet in powder; nor should the charge of any battering piece exceed $\frac{1}{7}$ of the weight of its bullet.

For if field-pieces of the customary length are loaded with $\frac{1}{6}$ of the weight of the ball in powder, I find this charge will communicate to the shot a velocity nearly approaching to that of 1200 feet in a second; provided the windage be moderate; which indeed in the greatest part of the modern pieces is exorbitant. Hence then, on our former principles, no greater charge should be allotted to any field-piece. And indeed when small grape are made use of, this is too much; for then even the half of it is sufficient; in firing against ships the powder ought never to exceed $\frac{1}{7}$ of the weight of the ball. For I have experienced, that an 18 pounder, with only 3lb. $\frac{1}{2}$ of powder, will traverse a butt of the stoutest soundest oak, whose thickness does not exceed 36 inches. It remains then only to consider, what is a proper charge for ruining defences and battering in breach. And here I am

I am confident, both from trials of my own, and from the experience of others, that $\frac{1}{3}$ of the weight of the bullet in powder is sufficient for piercing the stoutest masonry, with which any ramparts are faced, and though each shot separately examined will be less forcible with this proportion of powder, than with double the quantity (which is the charge recommended by many artillerists) yet the promptitude and facility of the service will be so much greater, that the effect of a day's battering will be more considerable with this charge than with any larger quantity of powder. Having thus dispatched the investigation of a proper charge, let us now examine the grounds, upon which some late artillerists have pretended to establish principles directly contrary to those, we have here advanced.

MAXIM XXI.

Although precepts very different from those we have here given, are usually advanced by artillerists, and are often said to be deduced from experience; yet is that pretended experience altogether fallacious; since from our doctrine of resistance established above, it follows, that every speculation on the subject of artillery, which is only founded on the experimented ranges of bullets discharged with considerable velocities, is liable to great uncertainty.

For we have seen, that, when the velocity of the bullet exceeds that of 1200 feet in a second; there will then correspond but very small differences in the regular ranges to very great differences in velocity. And even these small differences cannot be collected with any kind of accuracy; since the

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the regular ranges will frequently undergo much greater changes from the action of the deflective force, than the whole of those differences amounts to. And let it not be supposed, that this inconvenience can be prevented by making a number of trials, and taking the medium of them; for the irregularities in the repetition of similar trials are so very great, that the medium itself will vary considerably each time, that a new set of the same experiments are examined; and the inequality hence arising will often greatly exceed those differences, which these mediums are intended to investigate. As an instance of what we have advanced, we will here insert a set of trials made at *Woolwich* in the year 1736, when six 24 pounders were cast of nearly the same weight, but of different lengths. These were all loaded with the same charge, and were all elevated to $7^{\circ}\frac{1}{4}$; the length of each piece, and the medium of the ranges made with them on three different days, may be seen in the following table:

Length of pieces. Feet.	June 1st. Medium of five ranges. Yards.	June 18th. Medium of three ranges. Yards.	July 2d. Medium of three ranges. Yards.
$10\frac{1}{2}$	2486	2614	2406
10	2570	2532	2436
$9\frac{1}{2}$	2633	2560	2500
9	2790	2494	2563
$8\frac{1}{2}$	2586	2490	2466
8	2438	2473	2452

Here it appears, that the medium of the ranges with the same piece is different on different days; and this not the same way in all; but some of them are larger, when others are less. And this difference in the piece of 9 feet amounts to 296 yards, and in none of them, except the last, falls short of 120 yards. Now from principles, we have explained more at large in another place, it follows, that

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that the greatest inequality between the ranges of the longest and shortest of these pieces, supposing the shot to follow its regular track uninfluenced by the deflecting force, will be scarcely 70 yards: hence then it is evident, that these trials are altogether insufficient for investigating that inequality; since even the mediums themselves vary by above four times as much, as the whole of it amounts to.

It might have been expected, that a bare view of these ranges, as they are inserted in the preceding table, might have evinced the impossibility of deducing any certain principle from such discordant trials. But the force of prejudice was so great, that it was hence concluded, that the pieces of 9 and $9\frac{1}{2}$ feet in length carried their shot the farthest; and thence it was presumed, that those were the proper lengths of a 24 pounder, and that, whatever piece was either longer or shorter, was on that account less perfect, and impelled its shot with less violence. Indeed, as these cannon were cast on purpose to investigate, what were the proper lengths of pieces; it might not perhaps have suited the great apparatus of the inquiry, nor the dignity of the inquirers, to have acknowledged after all, that by the methods they pursued, the question could never be brought to a decision.

About three years after the experiments, I have just now recited, the same method of examination was prosecuted in *France*, with regard to the proper charges of pieces. And here again, by firing the same piece at a constant elevation, but with different charges of powder, it was pretended to be determined, that in most pieces, about a third of the weight of the bullet in powder was the proper charge, or the charge producing the greatest velocity. Of these trials I have seen a manuscript account; but the author has given only the mediums of the ranges; and he informs us at the same time, that he first rejected those, which appeared

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greatly irregular, though what those irregular ranges were, he has not told us. By methods like these, all those false persuasions, which we have hitherto combated, have been greatly countenanced; for the inequality of the ranges in the repetition of like trials being (as we have seen) so considerable, it was always in the power of a prejudiced artillerist to urge experiments in defence of his favourite hypothesis; since by neglecting those trials, which were inconsistent with his opinion, on pretence of their irregularity; it was not difficult, amidst the great variety of ranges, which arise from the repetition of the same experiment, to find such, as would agree to almost every notion, that hath at any time been started upon this subject of artillery.

It may perhaps be uged, that I myself have in other tracts compared the experimented ranges of projectiles with the deductions from my theory, and have urged their general agreement as a confirmation of the justness of my principles. And to this I answer, that, I conceive, I have established my theory upon more certain experiments; and that the only use, I make of the ranges of projectiles discharged with great velocities, is to shew, that they are more consonant to my doctrine, than they are to one another; and though the mediums of the ranges of those projectiles are not exact enough to evince the variation in the force of a shot arising from a small change in the length of the piece or in the charge; yet are they sufficient to shew the prodigious defalcation of the range by the resistance of the air, and to decide nearly, whether the law and quantity of that resistance be rightly determined by us or not.

Upon the whole then, as the writers on artillery in their speculations on the most eligible lengths of pieces, the proper quantities of powder, and the preference of particular practices, have prescribed

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prescribed no other method for the examination of their respective opinions, but the comparison of ranges made at the same elevation; we may thence conclude, that, when the velocity of the projectile was considerable, it was not possible they should have investigated these matters with any tolerable degree of justness: since we have shewn, that in great velocities the irregularities of the experiments, even when the mediums of a number of them are taken, do greatly surpass those differences, which the experiments are intended to discover. It is therefore not to be wondered at, if this vague method of examination, which, according as the trials were selected, might be urged in confirmation of different and even opposite assertions; it is not, I say, to be wondered at, that this loose and inconclusive experience hath been urged in support of the most erroneous opinions, and by its authority with those, who were ignorant of its fallacy, hath greatly tended to the establishment of the many prejudices and groundless persuasions, which at present prevail amongst the modern artillerists.

It is much to be lamented, that those who have been thus active in examining the ranges of pieces, for purposes which could not be answered thereby; should neglect to compare the same ranges with the established doctrine of projectiles. Had they done this, it would have been impossible, that the parabolic flight of shot, and the inconsiderable effect of the air's resistance, could have been so generally and strenuously maintained. Since how awkwardly soever the experiments upon ranges are adapted to the discovering the variations of great velocities; they are yet sufficient to demonstrate the monstrous errors in the common received doctrine, and to evince the extraordinary power of the air's resistance upon the most ponderous bodies. For the range even of a 24 pounder,

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der, fired with a usual charge at 45°, is but little more than a third of what, it ought to be on the parabolic hypothesis, if compared with the range at smaller elevations.

Having thus, as I conceive, evinced the inconclusiveness of the only species of experiments, which artillerists have hitherto prosecuted; it remains to point out a more indubitable method of examination, by which all precepts and practices in this art may be clearly and incontestably decided. This method (to which I have frequently appealed in the course of these maxims) requires no more than the ascertaining the depth, to which the bullet penetrates in some substance of a uniform texture. Indeed, if much nicety be required, it is always possible to determine the actual velocity of the bullet in different parts of its track, by practices I have explained at length in another treatise. But the application of this contrivance to large cannon, requires a very expensive apparatus; and therefore, since we are not now discussing this subject with geometrical rigour, we shall content ourselves with pointing out a more easy and expeditious procedure.

MAXIM XXII.

The depth to which a bullet penetrates in a solid substance, is a much more definite criterion of its comparative velocity, than the distance it ranges to when fired at an elevation. For, when the velocity of the bullet is doubled, it penetrates into a uniform substance near four times as deep, and with three times the velocity, near nine times as deep; so that with different velocities the penetrations vary in a much greater proportion than the velocities themselves.

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That the penetrations follow the rule laid down in this maxim, I have found by frequent experience. For instance, an iron bullet of $\frac{3}{4}$ of an inch diameter, when fired against a large block of sound oak, hath with different velocities penetrated to different depths, from less than $\frac{1}{2}$ an inch to ten inches; and the respective velocities being examined and compared with the corresponding penetrations, it was found, that all the experiments were sufficiently consonant to this rule. The same holds true in bullets of any size; for an eighteen pound shot, with a velocity of about 400 feet in a second, penetrated 3 inches $\frac{1}{2}$ into a butt of seasoned oak, and with about three times that velocity it penetrated 34 inches into the same wood.

This law holds either in large or small velocities; at least I have found it take place, when the bullet has moved at the rate of 2400 feet in a second; the reason of this relation between the velocity of the bullet, and its penetration, we have discussed at large in another place; but that is a speculation not essential to our present views; it is sufficient for our purpose, that the maxim itself is warranted by numerous experiments. However, not to leave the artillerist altogether to seek in this matter, I shall observe, that the resistance to a bullet in its penetrating a solid body, does not depend on the velocity of the bullet (very different in this circumstance from the resistance of the air) but is nearly the same, whether the bullet moves faster or slower. Whence it is not difficult to conceive, that since, when the velocity is increased, the resistance is not; therefore the penetration will be augmented in a greater proportion than the velocity.

MAXIM XXIII.

Hence then in all contests about the greate, or less velocity, which a bullet acquire,

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Again, if the various operations of gunnery require diversity of charges, and if the charges proper for different purposes are rightly assigned by us; then those practical artillerists, who have assigned the same proportion of the weight of the bullet in powder for every species of cannon, and for every employment of artillery, and who have been in general so strangely exorbitant in their determination of that proportion, may surely be accused of no small want of skill; and the practice itself in these particulars cannot but be condemned as extremely imperfect, and liable to the greatest exception, both with regard to its œconomy, its facility, and its dispatch.

And though our view in this essay is to animadvert on those faulty principles, which have hitherto prevailed, and to establish a juster theory in their stead; for which reason we shall avoid insisting on other mistakes, that are to be met with in the modern practice, as they are rather defects in the mechanical part, than errors arising from wrong speculation: yet at the same time we cannot help observing, that even the usual methods of training up those, who are intended for the service of the artillery, do not contain either the instruction or the variety of practice, which so important a business seems to demand.

I should therefore propose, that, instead of the puerile method of firing with a constant charge at two or three marks placed at known and invariable distances; a more extensive exercise should be introduced, in which the ranges, penetrations, and deflections of pieces, fired with all necessary diversity of charge, elevation, and distance, should be examined; and the result compared with the principles, we have above inculcated. And as we flatter ourselves, that this diversified experience would confirm and illustrate those principles; the practitioner would hence acquire a true scientific dexterity,

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dexterity, and would be hence prepared readily to adapt his charge, elevation, &c. to every emergency of actual service. Indeed it is high time that this most interesting art should be advanced to a state of more perfection; for it is matter of astonishment to consider, that, whilst other branches of mechanics have received such numerous improvements, and have been so successfully cultivated, merely by the industry and labour of private persons, who had rarely any other incitements than the impulse of their natural genius; yet this science of artillery, on which the success of military enterprizes, and consequently the fate of nations, often depends, hath still been obscured, either by the uncouth principles, which prevailed above two ages since; or by the more illusive refinements of modern theorists. And this too, whilst in almost every part of *Europe* a very large proportion of treasure hath been allotted for its support, and a numerous body of men received considerable largesses and emoluments for their supposed dexterity and skill therein. It must however be owned, that the present defects of this art are not to be solely imputed to the supineness and inattention of its practitioners; since before the discovery of those principles, which we some years since published to the world, and which we have more popularly explained in this essay; it was not possible for the most diligent examiner to extricate himself from the obscurity and confusion, which he was constantly involved in, when he repeated the same experiment a number of times successively. For the prodigious augmentation of the air's resistance in great velocities, and the deflective force arising from the whirling motion of the bullet (both which discoveries we claim as our own) may be considered as the two capital points, from whence the solution of almost all these wonderful occurrences in the ranges of projectiles may be de-

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duced, which, without the knowledge of these principles, appear little less than miraculous. But we apprehend we have been too copious upon this head already; and shall therefore now conclude these popular maxims.

A PROPOSAL

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A

PROPOSAL

FOR INCREASING

THE STRENGTH

OF

THE BRITISH NAVY, &c.

BY CHANGING ALL THE GUNS, FROM THE EIGHTEEN POUNDERS DOWNWARDS, INTO OTHERS OF EQUAL WEIGHT, BUT OF A GREATER BORE.

First printed in 1747.

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PREFACE.

THE following paper was drawn up near two years since; in consequence of some experiments and speculations of a much older date. As it is customary for mankind to suppose no one skilful in any profession, in which he has not been regularly initiated, and to which he is not formally attached; I did not expect, that this production, were it made public, would be considered otherwise than as the visionary notions of a speculatist, utterly unacquainted with the subject he had undertaken to discuss. Nor would it have been possible for me to have removed this prejudice by the most authentic proofs, I could have given, of my attention and industry in trying the various conclusions, on which the ensuing suggestions are founded.

But having lately been favoured, by the Honourable *George Anson*, Esq. vice-admiral of the *Blue*, and one of the lords of the admiralty, with the perusal of a *French* manuscript, taken on board the *Mars* man of war, I therein found recited a great number of experiments, extremely apposite to the principles

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principles inculcated in the ensuing paper; besides several important confirmations of a late reform in the service of the *French* artillery, analogous to what I have proposed. And therefore, as the authority of these *French* papers will, I hope, alleviate at least the censures, I might be otherwise exposed to; and as the particulars, they contain, are not, I think, unworthy public knowledge; I have, on this account, ventured to publish the following tract; and have annexed thereto, by way of annotation, such articles of this *French* manuscript, as, I conceived, would fully confirm the positions I had advanced, and would thereby render the matter, I have treated of, more worthy of future consideration; reserving till another time a more ample account of all these *French* experiments, and the various deductions founded thereon.

A PROPOSAL

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A

PROPOSAL

FOR INCREASING

THE STRENGTH

OF

THE BRITISH NAVY, &c.

THE advantage of large cannon over those of a smaller bore is so generally acknowledged, that a particular discussion of it might perhaps be spared; but, as it may be necessary to recur to the principles, on which this advantage is founded, in obviating some objections, which may perhaps be made to the present proposal; the author begs leave to enumerate the particular circumstances, in which heavy shot excel, together with the reasons of this superiority: and this, from the great number of experiments he has made on these subjects, he flatters himself, he is better able to do, than could be done by any one from the inaccurate observations of common practice only.

And first in distant cannonading: The resistance of the air to cannon-bullets, when they are fired with their usual allotment of powder, is so extremely great, that the distance, they range to at an elevation, is more regulated by the degree of this resistance, than by the velocity they receive from the powder. And the larger bullets being less resisted in proportion to their weight than the smaller, the distance, to which these larger bul-

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lets fly with the same proportion of powder, exceeds the flight of the smaller ones, almost in the proportion of their diameters; so that a 32 pound shot, for instance, being somewhat more than six inches in diameter, and a 9 pound but four inches; the 32 pound shot will fly near half as far again as that of 9 pound, if both pieces are so elevated as to range to the furthest distance possible.

And this advantage in the range of the heavier bullet is not easily to be counter-balanced by any extraordinary effort given to the smaller bullet, by increasing the quantity of its powder; for though the swiftness of the smaller bullet, at its issuing from the piece, may be thereby greatly increased, yet, as has been already observed, the distance to which the bullet flies at an elevation, is thereby but little influenced; for all this increase of celerity is presently taken away by the resistance of the air, which increases much faster. Thus, for instance, though a bullet fired with $\frac{2}{3}$ of its weight in powder has doubtless a greater velocity than the same bullet when fired with only $\frac{1}{3}$ of its weight in powder, yet if their ranges at ten or fifteen degrees are compared together, the certain difference between them will not be worth regarding; for it will not be more than the irregularities, which happen in repetitions of the same trial with the same piece, charge, and elevation.*

But

* This assertion, however strange it may appear at first sight, is irrefragably established by the experiments recited in the *French* manuscript mentioned in the preface; for there it appears, that when a 24 pounder was elevated to five degrees, and loaded successively with eight, nine, ten, eleven, twelve, fourteen, sixteen, eighteen, and twenty pounds of powder, the medium range with eight pounds of powder was eight hundred and forty toises, and the medium range with twenty pounds of powder was but nine hundred and seventy. Now the difference between eight hundred and forty, and nine hundred and seventy, is not so great as the difference, which sometimes occurred in these trials, on the repetition of the same experiment. I shall only add,

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But this circumstance of ranging farther in distant cannonading is perhaps the least considerable pre-eminence of heavy shot; for the uncertainty in this practice, especially at sea, is so great, that it has been generally discountenanced by the most skilful commanders, as tending only to waste ammunition. The most important advantage of heavy bullets is this, that, with the same velocity, they break out holes in all solid bodies in a greater proportion than their weight; that is, for instance, a 24 pound shot will, with the same velocity, break out a hole in any wall, rampart, or solid beam, in which it lodges, above eight times larger than will be made by a 3 pound shot; for its diameter being double, it will make a superficial fracture above four times as great as the 3 pounder (more of a smaller hole being closed up by the springing of the solid body than of a great one) and it will penetrate to more than twice the depth. By this means the firmest walls of masonry are easily cut through their whole substance by heavy shot, which could never be effected by those of a smaller calibre; and in ships the strongest beams and masts are hereby fractured, which a very great number of small bullets would scarcely injure.

To this last advantage of large cannon, which is indeed a capital one, there must be added that of carrying the weight of their bullet in grape or lead shot, and thereby annoying the enemy more effectually, than could be done by ten times the number of small pieces.

These are the principal advantages of large cannon, and hence it is no wonder, that those entrusted with the care of the *British* navy have always

add, that both the quantity and proportion of the ranges with eight, and with twenty pounds of powder, are extremely near, what I have long since computed them at; and that the whole of these experiments are entirely consistent with the theory both with the force of powder, and the resistance of the air, which I have some time since established.

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always endeavoured to arm all vessels with the largest cannon, they could with safety bear; and, indeed, within these last hundred years, great improvements have been made on this head, by reducing the weight of many of the species of cannon, and thereby enabling the same ships to carry guns of a larger bore: and very lately the 6 pounders in some of the smaller ships have been changed for 9 pounders of a lighter fabrick than usual, which hath been justly esteemed a very great addition to the strength of those vessels.

The importance then of allotting to all ships the largest cannon they can with safety bear, being granted; it remains to shew, on what foundation a change is proposed to be made in the fabrick of all pieces from the 24 pounders downwards; whereby all the guns from the present 18 pounders downwards, may be changed for others of the same or less weight, but of a larger bore. This proposition turns on the following considerations.

The species of cannon proper for each ship is limited by the weight of the pieces, and when the charge and the effort of the bullet are assigned, this weight in each piece is or ought to be determined by the following circumstances.

That they shall not be in danger of bursting.

That they shall not heat too much in frequent firing.

And that they shall not recoil too boisterously.

All this is to be done by a proper quantity of metal properly disposed; and when the pieces are secured from these accidents, all addition of metal beyond is not only useless but prejudicial.

Now what dimensions and weight of metal are more than sufficient for these purposes, we may learn from the present practice of the navy in the fabrick of the 32 pounders, the heaviest guns in common use; these are made to weigh, if the author's information be right, from 52 to 53 hundred weight;

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weight; that is somewhat less than a hundred and two thirds for each pound of bullet.

From this then the author concludes, that any smaller piece, made upon the model of these 32 pounders, and having their weight proportioned in the same manner to the weight of their bullet, will fully answer all the purposes recited above, and will be of unexceptionable service.

And he founds his opinion on these two principles.

First, That the strength of iron, or of any other metal, is in proportion to its substance; so that, for instance, where it has half the substance, it has half the strength; and this supposition he presumes, will be scarcely contested.

Secondly, That the force of different quantities of powder fired in spaces, which they respectively fill, is not exactly in the proportion of those quantities, but the lesser quantity has in proportion the least force: that is, for instance, the force of one pound of powder, in like circumstances, is less than half the force of two pounds. And this principle the author has deduced from many repeated and diversified trials of his own; and he believes, it will be found agreeable to all the observations, which have been made, or shall be made on this subject.

From these two considerations, he hopes, it will be granted him, that if two pieces, a large one and a small one, are made with all their dimensions in proportion to the diameter of their respective bullets, and consequently their weight in the same proportion with the weights of their bullets, then the larger piece, with the same proportion of powder, will be more strained, will heat more, and recoil more than the smaller.

Hence then, as we are assured, that the present 32 pounders are of a sufficient strength and weight for all marine purposes; we have the greatest reason

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son to suppose, that if all the pieces of an inferior calibre were formed upon the same model, measuring by the diameter of the bullet, these smaller pieces would not be defective in either strength or weight, but would be to the full as serviceable on shipboard as the present pieces, which are so much overloaded with metal.

The author's scheme then for augmenting the force of the present sea-batteries, is no more than this plain principle, that all ship-guns should be cast upon the model of the 32 pounders, measuring by the diameter of the respective bullet; so that for each pound of bullet there should be allowed one hundred and two thirds of metal only.

The advantages of this scheme will appear, by the following comparison of the weight of the present pieces, with their weight proposed by this new fabrick.

Pieces.	Weight now in hundreds.	Ditto by new fabrick.
24	48 to 46	40
18	41 to 39	30
12	34 to 31	20
9	29 to 26	15
6	24 to 18	10

Hence then it appears, that the 24 pounders will be eased of six or eight hundred of useless metal; and that instead of those of an inferior calibre now used, much larger ones of the same weight may be borne, especially when it is remembered, that this computation exceeds even the present proportion of the 32 pounders; so that from the above projected 18 pounders, for instance, two or three hundred may be safely taken.

The changes then proposed by the author are these,

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Pounders.		Hundreds.	Pounders.		Hundreds.
For 6	of	24 and 18	New 12	of	20
9		29 and 26	18		28
12		34 and 31	18		28
18		41 and 39	24		40

The 9 pounders lately cast, being, as the author is informed, still lighter than what is here represented, they may perhaps be only transformed into 12 pounders; but this will be a very great addition of strength, and the 12 pounders thus borne will be considerably lighter than the smallest 9 pounders now in use. The weight of the present 3 pounders are not remembered exactly by the author; but he doubts not, but they are heavier than the proposed 6 pounders, and may therefore be changed for them.

That many objections will be made to the present proposal, is not to be questioned; but as they will equally hold against the use of the present 32 pounders, which are known to be guns of unexceptionable service, that alone, it is conceived, will be an answer.

If it be supposed (as ancient practice is always favourably heard) that the excesses in the proportionate weight of the small pieces must have been originally founded on some approved principle, or otherwise they could not have been brought into use. It may be answered, that a hundred years since there were 4 pounders made use of, which were heavier than some of the present 9 pounders, and had the same prescription to plead in their behalf. Perhaps the origin of this excess in the smaller pieces may be accounted for by supposing, that when guns are used in batteries on shore, their length cannot be in proportion to the diameter of their bore; because the parapet being of a considerable thickness, a short piece would by its blast ruin the embrasures; and the smaller pieces,

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being for this reason made nearly of the same length with the larger, did hence receive their additional weight of metal. But this reason holds not at sea, where there is no other exception to the shortness of the piece, but the loss of force, which, in the instances here proposed, is altogether inconsiderable. For the old 12 pounders, for example, being in length from nine feet to nine and a half; the new ones here proposed will be from seven feet to seven and a half long. The difference in the force of the bullet, fired from these different pieces, is but little; and it will hereafter appear, that in the present subject much greater differences than these are of no consequence.

If it should be said, that the new fabric here proposed must have the present allowance of powder (which in the smaller pieces is half the weight of the ball) diminished, and that it must be reduced to the rate of 32 pounders, which is only seven sixteenths of the weight of the ball; it is answered, that if the powder in all ship-cannon whatever, was still farther reduced to one third of the weight of the ball, or even less, it would be a considerable advantage, not only by the saving of ammunition, but by keeping the guns cooler and quieter, and at the same time more effectually injuring the vessels of the enemy*; for with

* The change proposed here, of reducing the quantity of powder in all ship-guns to one third of the weight of the bullet, has for some time past been practiced by the *French* in a much severer service, where the increasing the velocity of the bullet could not at any time diminish its effect; the service, I mean, is battering in breach. For I learn from the forementioned *French* manuscript, that of late years all their breaches, in the different sieges they have undertaken, have been made with this very charge, that is, their 24 pounders have been loaded with eight pounds of powder, and they have found, that though the penetration of the bullet is less with this charge than with a larger one, yet the other conveniences, attending this smaller charge, are more than sufficient to balance that particular.

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with the present allowance of powder the guns are heated, and their tackle and furniture strained; and this only to render the bullet less efficacious than it would prove if impelled by a smaller charge. Indeed in battering of walls, which are not to be penetrated by a single shot from any piece whatever, the velocity of the bullet, how much soever augmented, still produces a proportionate effect, by augmenting the depth to which it penetrates: but the sides of the strongest ships, and the greater part of her timbers are of a limited thickness, insufficient to stop the generality of cannon-bullets, fired at a reasonable distance, even with a less charge than is here proposed. And it is a matter of experiment, that a bullet, which can but just pass through a piece of timber, and loses almost all its motion thereby, has a much better chance of rending and fracturing it, than if it passed through it with a much greater velocity.

That a better judgment may be made of the reasonableness of this speculation, the author thinks proper to add (and he believes future experience will not contradict him) that a 12 pounder, as here proposed, which is one of the smallest pieces at present under consideration, when charged with one third of the weight of the bullet in powder, will penetrate a beam of the best seasoned, toughest oak, to more than twenty inches depth; and

And here I must observe, that there have not been wanting persons of considerable name, who have asserted (as appears from the manuscript in question) that the velocity of a 24 pound bullet was really greater with eight pounds of powder than with any larger quantity; founding their opinion on the ridiculous persuasion, that whatever quantity was put in, no more than eight pounds of it took fire. But this position is destroyed by their own experiments, and their own reasonings; and later experiments, made with greater attention, put it beyond all doubt, that to the larger charge (at least as far as twenty pounds of powder) there corresponds a greater velocity.

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if, instead of one solid beam, there are a number of small ones, or of planks, laid together; then, allowing for the rending and tearing, frequent in such cases, he doubts not, but it will often go through near double that thickness, and this any where within a hundred yards distance: that is, any where within that distance, which the most experienced officers have recommended for naval engagements. In the same distance a bullet from the 12 pounders now in use, charged with half the weight of powder, will penetrate about one third part deeper: but if the efforts of each piece are compared together at five hundred yards distance, the differences of their forces will not be considerable. If this be so, it will not be asserted, I imagine, that the 12 pounder here proposed is less useful, or less efficacious, for all naval purposes, than the weightier 12 pounder hitherto made use of.

The author has in this proposal fixed on the 32 pounders as the standard for the rest; because they have been authorised by long experience. But from the trials he has made, he is well satisfied, a much greater reduction of weight, than he here proposes, might safely take place; and that one fourth, or even one fifth, of the weight of the bullet in powder, if properly disposed, is abundantly sufficient for every species of ship-guns*. However,

* This opinion is not advanced at random. I have myself procured to be cast a 4 pounder of iron, weighing about two hundred weight, which is not one third of the weight here proposed: As likewise two others, one of about three hundred weight, and the other about four hundred. That of three hundred weight, being fired with twelve ounces of common powder, went through, at the same time, two planks of very sound oak of four inches thick each, and a beam of the same fourteen inches thick, being in all twenty two inches, and afterwards buried itself in a bank of earth. And this it likewise did on a second

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However, the author is far from desiring, that his speculations should be relied on in an affair of this nature, where he pretends not to have tried the very matter he proposes, but founds his opinion on certain general principles and collateral experiments, which, he conceives, he may apply to the present case without error. He would himself recommend an experimental examination of this proposal, as the only one to which credit ought to be given. What he intends by the present paper, is to represent it as a matter worthy of consideration, and really such as it appeared to him. If those, to whose censure he submits it, are of the same opinion, there is an obvious method of determining, how far his allegations are conclusive; and that is by directing one of these pieces to be cast, a 12 pounder for instance, and letting it be proved with the same proportion of powder, allotted for the proof of the 32 pounders. Then if this piece be fired a number of times successively on a carriage, and its recoil and degree of heat be attended to, and if the penetration of its bullet into a thick butt of oak beams or plank be likewise examined; a judgment may thence be

 formed,

second repetition, when it broke off a piece of the beam of a quarter of a hundred weight, and drove it to above ten yards distance. The first and second of these pieces, on repeated trials, burst; but the last of four hundred weight continues still entire, and is, I conceive, as serviceable a field-piece as any whatever, notwithstanding its lightness. For with nine ounces of powder it throws its bullet point blank, as it is called, three hundred yards. And it will bear proving with twice or three times its proper charge. Indeed the other two, which were lighter, did not fail, I conceive, from a want of sufficient substance, but from a particular mistake in their fabric. However, the heaviest of them is but about one quarter of the weight, they are generally made of; and consequently, if capable of proper service, might in particular emergencies be of infinite use.

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formed, of what may be expected from the piece in real service; and the result of these trials will be the most incontestable confutation, or confirmation of this proposal.

A LETTER

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A Letter to Martin Folkes, Esquire, President of the Royal Society, in Answer to one of his, inclosing a written Message from the Chevalier D'Ossorio, Envoy from the King of Sardinia.

SIR,

I HAVE received a letter from you, inclosing a message, you have received from his excellency the Chevalier D'Ossorio, who, it seems, is desirous of having a copy of a paper presented to the Royal Society in relation to the proper charge of cannon: and which, you conceive, Sir, is my letter to Lord Anson of *March* last. As I esteem myself greatly honoured by this request; I cannot but desire, that his excellency might have a copy not only of that, but of any other papers of mine relating to this subject. For as his *Sardinian* majesty's curiosity appears to be founded on some experiments made by his order at *Turin;* I am willing to hope, that this enquiry may give rise to still more decisive trials. And that the doctrine, I have advanced, may thereby be at last established beyond the reach of contradiction. But as the consideration of the proper charge of cannon, to which this message relates, and about which the experiments at *Turin* have been employed, is a matter only occasionally mentioned in my papers; I beg leave to explain myself more at large to you on this head, than I have yet done; and to establish the true principles, on which this investigation ought to turn. This I shall undertake with more boldness, as I am confident, that

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it has not hitherto been understood. And that all the experiments, I have ever yet heard of, which were made with this view, are extremely fallacious, and incapable of determining the point in question. And I think, it will be no improper method of treating this subject, first to consider the erroneous maxims and inconclusive experiments, which have been usually adhered to; before I undertake to prescribe the genuine idea, which ought to guide us in the prosecution of the present enquiry.

Those, who have attempted to assign the proper charges of cannon, (and there are few artillerists, who have not been engaged in this attempt) those, I say, who have hitherto laboured in this matter, have usually supposed, that in every piece there was a certain charge of powder, which would give to the bullet a greater velocity than any other quantity of powder, either greater or less. And it hath been generally conceived, that this charge, producing the greatest velocity, was that, where all the powder took fire. Now in confirmation of these opinions, some of the more industrious artillerists have fired the same piece of cannon at an unvaried elevation, with different charges of powder gradually increasing in quantity; and they have pretended, that after using a certain charge, the range of the piece rather diminished than increased on augmenting the quantity of powder. Hence they have concluded, that this charge, which it was supposed ranged the bullet farther than any other quantity of powder would do, was the true and genuine charge of the piece in question. But these opinions are all entirely erroneous, and the experiments fallacious. For neither is there any such charge, as will give to the bullet a greater velocity, than would be produced by a larger quantity of powder (at least not within the limits where it has been usually supposed) nor has the compleat firing of the pow-

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der any thing to do with the determination in question. Nor do the experiments generally succeed in the manner above-mentioned. Nor can it be always concluded, that the bullet, which ranged farthest, was discharged with the greatest velocity; so that not only the principal suppositions, on which the investigation of this proper velocity hath hitherto been founded, are faulty; but all the superstructure is equally exceptionable. As, I hope, will sufficiently appear from the following considerations.

For as this proper charge, producing the greatest velocity, hath been usually supposed to be between two thirds of the weight of the bullet, and one third; I am fully satisfied, from numerous trials, that if, in small pieces, the charge be augmented greatly beyond these limits, the velocity will be thereby considerably increased. For instance, a common firelock, with a fit leaden ball of twelve to the pound, with a charge of one third of the weight of the bullet in powder, will be impelled with a velocity of about 1500 feet in 1", with a charge of half its weight in powder, its velocity will be about 1700 feet in 1"; with two thirds of the weight of powder, its velocity is about 1900 feet in 1"; but with twice the last charge, or one and a half the weight of the bullet, its velocity will be above 2300 feet in 1". And this last proportion of powder being double the largest quantity hitherto supposed for the proper charge, or for the charge productive of the greatest velocity; it hence follows, that all the former determinations about this matter are extremely erroneous.

If it should be said, that in larger pieces the same conclusions will not hold, but that there an increase of powder beyond the customary charge (for instance more than $\frac{1}{2}$ or $\frac{2}{3}$ of the weight of the bullet) will not augment the velocity; I answer, that, I well know, that in pieces, whose

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whose lengths bear a less proportion to the diameters of their bores, the variation of velocity in the preceding instances would be less considerable. But that even then there is an increase of velocity always attending the increased charge, for the truth of which I appeal to vulgar and obvious experiments. For instance, in the proving of pieces with the weight of the bullet in powder (as is sometimes the practice) it is always found, that the recoil and heat of the piece, and consequently the action of the powder on the bullet, is more forcible; and in this circumstance the increased action of the powder cannot but produce an increase in the velocity of the bullet.

To ascertain this point still more incontestably, I would advise those, who imagine, that the weight of the bullet in powder does not produce a greater velocity than that of half the weight, to fire a piece of cannon with these different charges against a bank of earth of a uniform consistence, or against a butt of timber, and examine the different penetration of the bullet, taking care that the object, whatever it be, may not be farther from the piece than fifty yards. This experiment I have tried with a small piece, with all the care I could, making use of bullets of iron, which were turned to the bore; and I found in a piece of about thirty diameters in length, (which is very near the proportion of a common six pounder) that, if I first fired it with half the weight of the bullet in powder, and measured the penetration into a beam of very tough oak placed about ten yards distant, I could afterwards, by augmenting the charge, make the bullet penetrate more than half as deep again.

This may suffice to evince the falsity of that commonly received principle, of there being a certain charge to every piece, which will produce a greater velocity than any other quantity of powder,

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der, either greater or less. Indeed I deny not, there is such a charge in every piece, but it is much more than any that is made use of, either for service or proof; and what no piece cast with the usual dimensions can sustain. For it is such a quantity of powder, as will fill up nearly $\frac{3}{8}$ of the piece; and consequently is too much to be ever applied for any practical purpose whatever; and therefore can be of no assistance in the examination of the present subject.

The very first supposition, then, on which the determination of the proper charge hath hitherto proceeded, being thus, as I concieve, overthrown; it remains to examine the experiments, to which the followers of this opinion have recurred in support of their doctrine: that is to say, the experiments, where the same piece, fired at a given elevation, hath ranged its bullet farther with this imaginary charge, than with a greater quantity of powder. And, with regard to this argument, I assert, that the whole tenour of it is fallacious, and has no other foundation but the irregularity of the ranges of the same piece, though charged and pointed in the same manner; which, as I have formerly explained to the society, is owing to the deflection of the bullet in the air from its original direction. Hence it happens, that the ranges are no measures of its original velocity; because sometimes by a favourable deflection the smaller velocity may range farther than the larger. And I assert, and have many experiments to produce in support of this assertion, that all the pretended proofs drawn from the experiments on the ranges of cannon, are made out by taking those ranges only, where the deflection was favourable to the hypothesis in question, neglecting at the same time as irregular, those where the result was contrary to it. For the whole strength of this reasoning has generally turned upon a difference

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of 100 or 200 yards in the ranges. And I have by me a set of experiments made, as I conceive, with sufficient care, where a 24 pounder, elevated at $7^{\circ}\frac{1}{4}$, and charged each time with the same quantity of powder, hath in one shot ranged its bullet 2300 yards, and the very next time the range hath been more than 3000 yards. So that the range, with the same piece, elevation, and charge, doth sometimes differ four or five times as much as the casual differences, on which the opinion, here opposed, is pretended to be founded.

If then the very idea of a proper charge, considering it as productive of the greatest velocity, is itself chimerical within the limits of any practical determination; and if the experiments, urged in support of it, are such, as naturally arise from the necessary irregularity, which always occurs in the comparison of distant ranges; it may perhaps be asked, on what principle the proper allotment of powder can be assigned, or what circumstances there are, that can at any time determine the proportion necessary to be observed between the weight of the bullet and the charge of powder.

And to this I answer, that the only maxim, which can assist in fixing such a determination, is this: that every effect to be produced by the use of fire-arms, should be executed with the least powder possible, not only as hereby much ammunition may be saved, but also because with smaller charges the piece is less heated and strained, the service of it is more prompt, and the flight of the shot is much more steady.

Now from this maxim it follows, that, according to the different purposes, to which artillery is applied, different charges ought to be made use of. For instance, the resistance of the masonry, with which the ramparts of fortified places are usually faced, being much greater than that of the

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sides of the stoutest ships; the allotment of powder for battering in breach, and for firing against shipping, ought to be different. And the resistance of human bodies being still greatly short of either of these; all field-pieces, or such, as are used against troops, ought to be charged with a yet smaller proportion of powder. And though in these cases there is no absolute invariable standard to be assigned; yet I conceive, that something not very distant from the truth may be deduced from considering the limits, within which all the three forementioned operations are contained.

For as to battering in breach, I well know, that in most of the sieges, which the *French* have lately undertaken, they have succeeded extremely well with one third of the weight of the bullet in powder. Indeed the difference in force betwixt a bullet fired with one third of its weight in powder, and two thirds, is so very little, that the other advantages of the smaller charges greatly overbalance it; and therefore it ought to be allowed as an invariable rule, never to exceed this proportion of one third of the weight of the bullet in the service of any piece of artillery whatever; no not under any pretence of the great distance of the object. For I know, that, in great distances, the augmenting the quantity of powder is only an imaginary advantage.

With respect to the firing at ships, especially with heavy cannon, I could wish the charge was still farther diminished to a fourth or fifth; for I am satisfied from my own experiments, that (not to mention the other advantages) the havock made by bullets fired with those smaller quantities, would be much greater, than what is produced by the enormous charges now in use.

And lastly, as to the firing against troops in the open field, it is difficult to conceive, how very small

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small a proportion of powder will give to a bullet such a force, as will render it mortal to those it strikes. For it appeared by experiments formerly exhibited before the society, that an ounce of powder in the chamber of a small mortar, threw a 24 pound shot to above 240 yards. This bullet then received from the action of the powder as great a force, as it would have acquired by falling from a height equal to that of St. *Paul's* church. And it is sufficiently obvious, that a heavy bullet, let fall from such an eminence, must have killed any person it had fallen on. And lest it may be supposed, that in this case the weight of the bullet might contribute more to the stroke than its celerity, I must add, that a fit bullet, fired from a musket of the common calibre with $\frac{1}{24}$ part of its weight in powder, will go through a plank of fir of above an inch thick, and will doubtless produce a mortal wound in any vital part of the body. For it will penetrate into any soft substance near ten times deeper than a bullet discharged from the best cross-bow, I have ever yet met with, which yet is usually esteemed a mortal weapon. And comparing together the various experiments I have made, and the different circumstances necessary to be considered, I am fully satisfied, that no field-piece, whether fired with round-shot or with grape-shot, ought ever to be allowed more than $\frac{1}{7}$ of the weight of the bullet in powder, and when troops are near, and grape-shot is of use, even $\frac{1}{10}$ of the weight of the bullet will do great execution. For by large quantities of powder, grape-shot is often dispersed, and rendered ineffectual; whereas with smaller charges it flies more steady and compact, its direction can be more relied on, and the ravage it makes amongst troops is more terrible by its falling directly upon one particular part, and thereby opening the line. And this smallness of the charge too, requisite for field-pieces, when the reasons

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reasons of it shall be once well understood, and the practice established, will enable them to be made extremely light, and will thereby render their transportation and service much more easy, than it has been hitherto; and will likewise produce many other advantages not necessary to be here recounted.

I am sensible, that with respect to the diminishing the weight of field-pieces, many attempts have been made towards it within the last fifty years. But the general mistake of those, who have engaged in it, hath been either the serving these lighter pieces, with the same large charge allotted to the heavy ones; or if they did diminish the charge, the insisting that with this diminished charge the bullet was not so strongly impelled as with a greater quantity of powder. But this last assertion, as it was an insupportable falsehood, laid them under great difficulties, and gave the officers of artillery (who, from a religious attachment to the practice of their predecessors, were more usually their declared enemies) great advantages over them.

If it should be urged, that though the small charges, I have here recommended, are sufficient for doing execution near at hand; yet they are no ways adapted to the purpose of distant cannonadings: I answer, in the first place, that these distant cannonadings are rarely of consequence, and therefore are not properly the considerations, on which the present matter ought to be decided. But farther I assert, that the difference in the ranges, or in the force of the bullets at a distance, when fired with $\frac{1}{7}$ of their weight of powder, for instance, or with $\frac{1}{2}$, is too inconsiderable to merit attention. For from the principles I have formerly established, it follows, that a 4 pounder elevated to 10°, and fired with the smaller of these charges, ought to range about a fifth part short of what it

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would do when fired with the larger, and at the end of these respective ranges the difference in the velocities of the bullet will be insensible.

This, sir, is the substance of what hath occurred to me, on the present subject; every part of which, should it be doubted, I can undertake to evince by unquestionable experiments. If this letter appears too prolix, considering the great simplicity of the subject, I must excuse myself by observing, that it is a misfortune common to all those, who have first reformed any branch of science, to have been obliged to waste great part of their time, and to employ many arguments in rooting out the prejudices, which they found already established; although these prejudices had no other foundation but the reverence they had acquired from age, and from the habitual assent, they had been long received with. Indeed I am myself so fully persuaded of the difficulty of confuting long confirmed prepossessions, that, notwithstanding all that hath been already said, I cannot finish this letter without a short recapitulation of what I have here advanced. Which is, that the hypothesis of a proper charge, considering it as what would produce the greatest velocity, is chimerical; that the experiments urged in support of it are fallacious. That the greatest velocity, if it were attainable, is not the most eligible, it being often less efficacious than a smaller degree; that the only proper principle for determining the most convenient charge, is that of using the least powder possible with regard to the particular purpose in view. Therefore, that the charges ought to vary according to the different services. That in almost every modern practice, the charges are too great. And that in firing against troops in particular, bullets impelled with velocities greatly short of those now established, are to the full as effectual. That consequently in this case the charges

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charges may be extremely diminished. And thence the whole establishment of the modern field trains ought to be entirely changed. I shall only add, that the speculations of artillerists on the proper lengths of pieces have been embarrassed with errors of a like nature with those I have already censured, and that the decision of that matter can never be attained on the principles they have assumed.

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A

LETTER

to

LORD ANSON.

My Lord,

HAVING principally by your favour procured leave to make some trials with large cannon at *Chatham*, I think it my duty to lay the result of them before your lordship, especially as the subject of them relates to a matter about which I have formerly troubled your lordship; I mean, the diminishing the allotment of powder for heavy cannon, and thereby facilitating the reduction of the weight of those pieces.

And that I may not be misunderstood upon this head, I beg leave to explain in a few words the maxims I have formerly advanced, and which I conceive are fully confirmed by my late trials. This is the more necessary for me to do: since I have found the prejudices, and the erroneous opinions at present prevailing among the practitioners of artillery, have by mistake been often blended with what I have advanced upon this subject; and I have thereby had positions imputed to me, which were directly contrary to what I have always maintained.

To begin then; although I am satisfied, that no charges of powder, either now, or at any other time,

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time, in common use, are so great, but that by augmenting their quantity, some addition will be made to the velocity of the shot, and to the effort of the bullet near at hand; yet in large charges, I affirm, that the addition of the force of the bullet is very small in comparison of the increased proportion of the charge, and even this small addition of force is presently taken away by the vast resistance of the air to great velocities; whence neither the distance, to which the bullet flies at an elevation, nor the force of the bullet at the end of its flight, is sensibly augmented by very great augmentations of the quantity of powder. I affirm too, that in many instances the increasing the velocity of the bullet is not only an useless, but a prejudicial practice; since, in penetrating solid bodies, that bullet which has but just force enough to go through, will produce much greater effect, than a bullet, which has a considerable velocity left after it has got through. I must farther add, that both the ranges and effects of bullets, fired with small proportions of powder, do much surpass the expectations of all those, whether artillerists or others, who have not made the requisite experiments upon this subject.

These, my lord, are the hypotheses, which, as your lordship knows, I have always maintained, and are the positions I have had principally an eye to, in my late experiments at *Chatham*, where, on the 11th of *July* last, I got an 18 pounder on shore, which was the largest piece I could procure. This piece had its breech laid down as low as its carriage would permit, (when it was elevated somewhat short of 15°) and then instead of 9 pound of powder, which is its customary charge, it was tried with $\frac{1}{4}$ of a pound only, with which allowance it ranged the bullet, in several trials, from 220 to 250 yards. It was observable, that when the bullet lighted on dry firm ground, it

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rose again, and bounded on to a considerable distance; but meeting once with a small bank of meadow ground, it penetrated near three feet deep into it.

By comparing together the best accounts of the ancient military machines, I conceive, the velocity, with which their charges were projected, rarely exceed the velocity of the bullet fired with the smallest pittance of powder.

The next trial was with $\frac{1}{4}$ a pound of powder, or with the $\frac{1}{11}$ part of the usual charge, and the elevation now but 12°. This bullet ranged 500 yards, where grazing, it bounded on to near 300 yards farther.

Trying now with 1 pound of powder, or the $\frac{1}{5}$ of the customary charge, and elevating the piece to near 15°, the bullet ranged from 1400 to 1600 yards; after which, the elevation being diminished to 5°, it ranged from 550 to 630 yards.

With 2 pound of powder, at only $3°\frac{1}{2}$ elevation, the bullet ranged from 900 to 1100 yards, when at 15° it would have ranged to a mile and a half.

With 3 pound, and an elevation of 6°, the bullet ranged from 1500 to 1650 yards; and with $3\frac{1}{4}$ pound of powder, and the same elevation, ranged twice together to 1760 yards, or an *English* mile just. From which experiments, I conclude, that at 15° it would have flown at least 3000 yards.

These were the principal trials with regard to the ranges of an 18 pound bullet, with small charges of powder. These ranges, I conceive, will be acknowledged to exceed the expectations of those, who have not been conversant in trials of a like kind; and the ranges in the two last trials, with $3\frac{1}{4}$ pound of powder at 6°, would not be increased more than 200 yards by an addition of 7 pound more of powder; as I can easily evince by experiments which I have in my possession.

The

The next experiments were made to examine the penetration of bullets, fired with these small proportions of powder, in masses of timber. For this purpose, a butt of about 5 feet square, was framed of two sheets of the toughest, driest oak plank; the first sheet consisted of planks set perpendicularly in the ground: But in the second the planks were laid horizontally, each plank was $6\frac{1}{4}$ inches thick, so that the whole butt was 13 inches thick, and was bound together by cross pieces, which were well supported by props both before and behind. It was placed about 30 yards from the muzzle of the 18 pounder.

With 1 pound of powder, the bullet in repeated trials always passed through the whole butt, penetrating the first sheet of oak in a hole, which was free from splinters, but splintering the second sheet greatly, and driving the splinters from 10 to 30 yards distance.

The first butt being ruined, another was made of five thicknesses of plank instead of two, so that it was $32\frac{1}{4}$ inches through; the doubles here were placed perpendicular and horizontally alternately, each plank was trunnelled with three trunnels to that next behind it, and the whole was bound together by cross pieces, and was most firmly shored on both sides.

The 18 pounder, successively charged with $3\frac{1}{4}$ pound, with 3 pound, and with $2\frac{1}{4}$ pound of powder, the bullet in each shot went through the butt, driving every time great quantities of splinters before it; but the last shot of $2\frac{1}{4}$ of powder made much the greater ravage; for it drew the trunnels, and separated the doubles of plank from each other, and broke the hindermost plank (which was $6\frac{1}{4}$ inches thick, and 15 inches broad) short in two.

As I was aware, that the resistance of these doubles of plank, was less than that of large solid

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beams of timber; I directed a third butt to be made of dry seasoned beams of *English* oak; these beams were about a foot and a half thick, and about two feet broad. Three of them were set perpendicularly into the ground close to one another; then three more were laid on each other horizontally for a second row; and three more were set perpendicularly behind; the butt thus formed was $4\frac{1}{2}$ feet thick, and was bolted through with iron bolts of $1\frac{1}{4}$ inch diameter; it was besides strongly shored both before and behind.

The 18-pounder fired with 6 pound of powder, the bullet in several shots penetrated into this mass of timber from 37 to 46 inches deep.

With 3 pound of powder the penetration was near 33 inches.

With $2\frac{1}{2}$ pound of powder the penetration was 28 inches.

With one pound of powder the penetration was from $14\frac{1}{2}$ to $15\frac{1}{2}$ inches.

I must observe, that in all these instances great care was taken, that each shot should be planted in a fresh sound part of the butt, where the timbers had not been injured by the preceding trials; and I cannot but take notice, that the iron bolts, which bolted the butt together, were bent by shaking of the beams, as if they had been of small wire only.

It may perhaps be objected, that the distance of my butt from the piece was too little, and that had it been removed farther off, the force of the bullets fired with their small charges would have been much short of what I have described; if this should be urged, I answer, that I was obliged to place the butt nearer than I would have done, on account of the ground, which was a morass, passable only in a few places. However, I am well satisfied, that in much greater distances from the piece, the penetrations would not have been sensibly

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sibly short of what they came out in these trials. For firing 3 pound of powder at a firm bank of earth, which was 700 yards distant, the bullet went through it, where it was eight feet thick. I must add, that no endeavours were at any time used to augment the force of the powder; for the bullet was always thrust into the cartridge close upon the powder, and then the whole was put up the piece together without ramming, and without any wad either upon the powder or the bullet.

From all these experiments, I hope, my lord, I may conclude, that small charges are much more efficacious, than has been generally believed; that after a certain charge (for instance, $3\frac{1}{2}$ pound of powder in an 18 pounder) all addition of powder will create but an inconsiderable change either in the range at an elevation, or in the force at a distance: And that the penetration of an 18 pounder with 3 pound of powder, is more than sufficient for traversing the sides of the stoutest ships. The deductions from these principles are very numerous, and may prove of most extensive use in the service of artillery, both at sea and on shore; but as I have already trespassed so long on your lordship's patience, I shall refer these considerations, together with an account of the trials of another nature, which I made at the same time at *Chatham*, to a future disquisition.

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On pointing, or the directing of Cannon to strike distant Objects.

THE art of pointing of cannon, so as to strike distant objects, depends upon two things; the first of which, is the tracing on the outside of the piece a visual line parallel to the axis, by which means the piece is to be directed in all small distances of the objects; and the other is, the determining the allowance to be made in distant shot, for the incurvation of the flight of the bullet.

The first of these is usually called disparting, and is performed by taking half the difference of the diameters of the muzzle and base ring, and setting it perpendicularly on the muzzle ring directly over the centre; for then a line, which passes from that point in the base ring, which is directly over the centre of the piece, to the extremity of the distance thus placed on the muzzle ring, will, when the piece is truly bored, be parallel to its axis; and consequently in small distances, where no allowance is to be made for the incurvation of the shot, this will be the proper visual ray, for pointing the piece; and even in distant shot, where allowances are necessary, those allowances cannot be regulated, till this line be first assigned; for, as the object is more distant, the more must the piece be elevated above the line drawn from the piece to the object. What this elevation is, in regard to the different distances of the object, and how to be estimated, I shall now proceed to shew.

The

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The incurvation of a shot towards the ground in its flight, is *(cæteris paribus)* greater or less, according to the different charges of powder made use of: But as, for reasons explained in another place, we have supposed, that such a charge, as produces a velocity between 1100 and 1200 feet in a second, is in many cases the most eligible; we shall therefore take it for granted, that the pieces we are now considering, are discharged with such a quantity of powder, as will nearly produce that velocity. And then, if the shot were not retarded by the resistance of the air, the range corresponding to one degree of elevation of this piece might be reckoned 450 yards; to two degrees 900; to three degrees 1350; and so on; for, independent of the air's resistance, the ranges at different elevations would be nearly in proportion to those angles, at least as far as eight or ten degrees. But the resistance of the air will alter the case considerably; for that resistance, by perpetually diminishing the swiftness of the bullet, will occasion its track to be more incurvated, than would otherwise happen, and consequently the ranges will be diminished. And this diminution of the range will be greatest in the smaller shot, as they are more powerfully retarded. And as the computation of these varieties may be thought too intricate to be perpetually recurred to, in order to avoid that trouble, I have inserted in the annexed table, the angles of elevation which, in different pieces, correspond to different distances.

Actual

Actual range in yards	Angle of elevation.				
	24lb.	12lb.	9lb.	6lb.	3lb.
1400	4 12	4 30	4 45	5 0	5 57
1200	3 26	3 40	3 50	4 0	4 30
1000	2 45	2 54	3 0	3 7	3 30
800	2 6	2 10	2 13	2 20	2 30
600	1 30	1 32	1 34	1 36	1 44
500	1 13	1 15	1 17	1 19	1 22
400	58	58½	59	1 0¼	1 3
300	43	43	43+	44	43+
200	27¾	28¼	28½	28¾	29¼
100	14—	14+	14¼+	14½	15

By this table, when the distance of the object is known, the angle, by which the axis of the piece ought to be elevated above the visual line, or the line drawn from the piece to the object, will be readily determined. For whether the object be above or below the piece, need not be considered; since the range will always depend upon the angle by which its axis is elevated above the visual line; unless the inclination of that line to the horizon be much greater than usually occurs in practice. Now when the angle of the axis with the visual line is known, the piece is easily directed in the following manner.

Measure the length of the piece from the middle of the base ring to the middle of the muzzle ring, and compute the tangent to the given angle of elevation to that radius; this may either be done by tables of tangents, or sufficiently exact for our purpose by supposing that to a radius of 1 foot, $\frac{21}{100}$ of an inch is the tangent of a degree. This tangent being found, suppose* CHBD to be the given piece,

* Plate II. Fig. 11.

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piece, AB its axis, DE half the difference between the diameter of the muzzle ring and base ring, set upon the muzzle ring according to our first directions, so that CE may be the line of dispart; if now CG be erected upon the base ring, equal to the tangent of elevation just found, then the line GE will be the proper line for the pointing of the piece; for, when that line is directed to the object, the piece is justly laid.

If the directions here given are literally followed, there will be two perpendiculars erected, one upon the muzzle ring, and one upon the base ring, but in practice one is sufficient; for if GC exceed DE, that difference only, erected upon the base ring, will with the muzzle ring at D, give the line of direction; and if DE exceeds GC, the same difference erected on the muzzle ring, will in like manner with the base ring at C, give the directions of the piece.

I find, that the charge of powder producing such a velocity, as I here suppose to be the most eligible, is from $\frac{1}{6}$ to $\frac{1}{4}$ of the weight of the bullet in powder, the different goodness of powder, and the difference in the lengths of pieces producing that diversity. And though, by a variety in the charge, the velocity should not be exactly what is here supposed; yet in distant objects (where the principal difficulty lies) the inequality arising from thence will not be remarkable; or if it be, it may be easily rectified after the success of three or four shot have been attended to.

I shall only add, that in distances not exceeding 500 yards, it is not necessary to recur to the table inserted above; nor to make any difference in the pointing of the different pieces. For if in those short distances $\frac{1}{4}$ of a degree be supposed to be the inclination corresponding to every 100 yards, the result will in all kinds of cannon be sufficiently near for practice; as will readily appear

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pear to those, who shall compare the inclinations deduced from this rule, with the angle inserted in the table.

Here ends Mr. *Robin's* manuscript. I am sensible he wrote other tracts on this subject of gunnery, besides these I now publish. In particular, on the 9th of *April* 1747, there was read before the Royal Society a Letter of his to admiral *Anson*, on occasion of the manuscript put into his hands by the admiral, which was taken on board the *French* ship the *Mars*: and also, on the 2d of the following *July*, a Dissertation of his concerning the nature and advantage of rifled barrel pieces. This I have heard much commended by several gentlemen, who were present at its reading.

From his memorandums, I find, he had made many experiments on these sorts of barrels during the month of *March* in 1745; and to a rough draught of a Discourse on the irregularities shot were liable to (which irregularities he has fully described at page 196, &c. of the foregoing tracts) he subjoined what follows.

"I must add, that the sole advantage of rifled "pieces, is their preventing this irregularity; for "by the spiral turns of the rifle, closely confining "the bullet, they give it a rolling motion round "an axis, which is coincident with the line of its "direction, by which means its resistance is e-"qual on every part of the surface, that goes "foremost; and if a small inequality should at "any time intervene, it will be presently rectifi-"ed by that part shifting to the contrary side of "the axis.

"And this is agreeable to what has been uni-"versally practised in relation to arrows; for the "feathers of an arrow (as is well known to arch-"ers) are always placed in a spiral form, so as to "make an arrow spin round its axis, without

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" which their eye-sight would inform them, that " the arrow undulated in the air, and did not " keep accurately to its direction. This principle " is confirmed too by the necessity, which every " school-boy finds himself under of making his " shuttlecock spin.

" I have made some experiments on simpler " methods of performing this, and applicable to " iron bullets; my success as yet has not been " what I could wish; but it has however been " sufficient to encourage a farther prosecution, " which if I shall ever pursue farther, I know " not."

Besides, in a Letter Mr. *Robins* wrote to *Alexander Hume*, Esquire, dated *Fort St. David*, 18 *October* 1750, there is the following passage communicated to me by *Charles Frederick*, Esq. surveyor general of his Majesty's ordnance.

——" If you remember what passed in con- " versation with Mr. *Frederick* in relation to light " pieces of cannon; you will not be displeased to " hear the result of some trials, I have made since " my arrival here. These trials were made with " two six pounders, both of them elevated to 11°. " One of these was a heavy piece, weighing above " 21 hundred weight, and about eight feet in " length. The other was only four feet three " inches long, and weighed but five hundred " weight. The short piece being loaded with $1\frac{1}{4}$ " pound of powder, and the long piece with three " pounds, and four shot being fired from each; " it was found, that the two most distant ranges " of the short piece exceeded the two least ranges " of the long piece, and that the medium of the " ranges with the long piece exceeded the medi- " um of the ranges with the short piece, by little " more than 300 yards in 2500 yards. All which, " I believe, Sir, you will, on recollection, find " to be consonant to what I asserted at your house.

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" house. I hope to procure some iron pieces near-
" ly as light as the abovementioned six pounders.
" These pieces in this country being of infinite
" service in the field."

I shall conclude this volume with two discourses, on the height to which rockets ascend; the first written by Mr. *Robins*, the other by his friend Mr. *Ellicot*.

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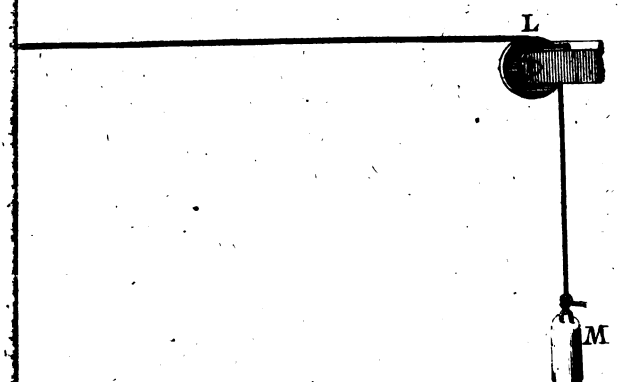
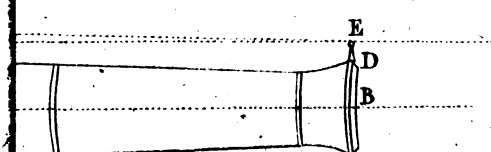
Pl. II *Page 318.*





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Observations on the Height to which Rockets ascend. Read before the Royal Society, May 4, 1749, and published in the Philosophical Transactions, Numb. 492.

THE use of rockets is, or may be, so considerable, in determining the position of distant places to each other, and in giving signals for naval or military purposes; that I thought it worth while to examine, what height they usually rise to, the better to determine the extent of the country, through which they can be seen. I therefore, at the exhibition of the late fireworks, desired a friend of mine, who I knew intended to be only a distant spectator, to observe the angle of elevation, to which the greatest of them rose; and likewise the angle made by the rocket, or rockets, which should rise the highest of all.

My friend was provided with an instrument, whose radius was 38 inches; and, to avoid all uncertainty in its motion, it was fixed in an invariable position; and its field, which took in ten degrees of altitude, was divided by horizontal threads. The station my friend chose, was on the top of Dr. *Nisbett's* house in *King-street* near *Cheapside*, where he had a fine view of the upper part of the building erected in the *Green-park*. There he observed that the single rockets, which rose the most erect, were usually elevated at their greatest height about $6°\frac{1}{4}$ above his level; and that amongst these there were three, which rose

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rose to $7°\frac{1}{2}$; and that in the last great flight of rockets, said to be 6000, the crest of the arch, formed by their general figure, was elevated about $8°\frac{1}{4}$. From the care and dexterity of my friend, and the nature of the instrument, I doubt not but these observations are true, within a few minutes.

The distance of this station from the building in the *Green-park* is 4000 yards, according to the last great map of *London*: and hence it appears, that the customary height, to which the single, or honorary rockets, as they are styled, ascended, was near 440 yards; that three of these rose 526 yards; and that the greatest height of any of those, fired in the grand girandole, was about 615 yards: All reckoned above the level of the place of observation, which I esteem to be near 25 yards higher than the *Green-park*, and little less than 15 yards below the chests, whence the great flight of rockets was discharged.

It seems then, there are rockets, which rise 600 yards, from the place whence they are discharged: And this being more than a third part of a mile, it follows, that, if their light be sufficiently strong, and the air be not hazy, they may be seen in a level country at above 50 miles distance.

The observations on the single rockets are sufficiently consonant to some experiments I made myself, about a fortnight since: for then I found, that several single pound rockets went to various heights, between 450 and 500 yards; the altitude of the highest being extremely near this last number, and the time of their ascent usually short of seven seconds.

But though from all these trials it should seem, as if good rockets of all sizes, had their heights limited between 400 and 600 yards; yet I am disposed to believe, that they may be made to reach much greater distances. This I in some degree collect

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collect from the nature of their composition, and the usual imperfect manner of forming them.

Nor is this merely matter of speculation; for I lately saw a dozen of four pound rockets fired; the greatest part of which took up near fourteen seconds in their ascent, and were totally obscured in a cloud near nine or ten seconds of the time; so that the moment of their bursting was only observable by a sudden glimmering through the clouds. And as these rockets, during the time they were visible, were far from moving with a languid motion; I cannot but conceive, that the extraordinary time of their ascent must have been attended by a very unusual rise.

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An Account of some Experiments, made by Benjamin Robins, *Esq. F. R. S. Mr.* Samuel Da Costa, *and several other gentlemen, in order to discover the Height to which Rockets may be made to ascend, and to what distance their Light may be seen; by Mr.* John Ellicott, *F. R. S. Read before the Royal Society* December 13, 1750, *and published in the Philosophical Transactions, Number* 496.

SOON after the exhibition of the fire-works* in the *Green-park*, Mr. *Robins* communicated to this Society an account of the height, to which several of the rockets there fired were observed to rise. In this account, after having given a short description of the instrument with which the heights were measured, he observes, that the customary height, to which the single or honorary rockets, as they are styled, ascended, was about 465 yards; that three of them rose to about 550 yards; and the greatest height of any of those fired in the grand girandole was about 600 yards. He likewise further observed, that, supposing rockets are made to ascend 600 yards, or more than a third of a mile, it follows, that, if their light be sufficiently strong, and the air not hazy,

* On occasion of the late peace.

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hazy, they may be seen in a level country at about 50 miles distance; and that from the nature of the composition, and the usual imperfect manner of forming them, he was of opinion, that rockets were capable of being greatly improved, and made to reach much greater distances.

Mr. *Robins* not having been able to obtain any certain account to what distance any of these rockets were actually seen, and considering the great use that might be made of rockets in determining the position of distant places, and in giving signals for naval and military purposes, he resolved to order some rockets to be fired at an appointed time, and to desire some of his friends to look out for them at several very distant places.

The places fixed upon for this purpose, were *Godmarsham*, in *Kent*, about 50 miles distant from *London*; *Beacon-Hill*, on *Tiptery-Heath*, in *Essex*, at about 40 miles; and *Barkway*, on the borders of *Hertfordshire*, about 38 miles from *London*.

Mr. *Robins* accordingly ordered some rockets to be made by a person many years employed in the Royal Laboratory at *Woolwich*; to which some gentlemen, who had been informed of Mr. *Robins's* intentions, added some others of their own making. The 27th of *September*, 1748, at eight in the evening, was the time appointed for the firing of them; but through the negligence of the engineer, they were not let off till above half an hour after the time agreed upon. There were in all a dozen rockets fired from *London-Field* at *Hackney*, and the heights were measured by Mr. *Canton*, Mr. *Robins* being present, at the distance of about 1200 yards from the post from whence the rockets were fired. The greatest part of them did not rise to above 400 yards; one to about 500, and one to 600 yards nearly.

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By a letter I received the next day from the Reverend Dr. *Mason* of *Trinty-College, Cambridge*, who had undertaken to look out for them from *Barkway* on the borders of *Hertfordshire*, I was informed, that, having waited upon a hill near the town with some of his friends till about half an hour past the time appointed, without perceiving any rockets, as they were returning to the town, some of the company seeing through the trees what they took to be a rocket, they immediately hastened back out of the closes into the open fields, and plainly saw four rise, turn, and spread. He judged, they rose about one degree above the horizon, and that their lights were strong enough to have been seen much further.

From *Essex* I was informed, that the persons on *Tiptery-Heath* saw eight or nine rockets very distinctly, at about half an hour past eight; and likewise greatly to the eastward of those five or six more. The gentlemen from *Godmarsham* in *Kent*, having waited till above half an hour past eight without being able to discern any rockets, they fired half a dozen, which from the bearings of the places were most probably those seen to the eastward by the persons upon *Tiptery-Heath;* and if the situations, as laid down in the common maps, are to be depended upon, at about 35 miles distance.

The engineer being of opinion, that he could make some rockets of the same size as the former, that should rise much higher, Mr. *Robins* ordered him to make half a dozen. These last were fired the 12th of *October* following, from the same place, and in general they rose nearly to the same heights with the foregoing; excepting one, which was observed to rise 690 yards. The evening proved very hazy, which rendered it impossible for them to be seen to any considerable distance.

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It being observed in these trials, that the largest of the rockets, which were about two inches and a half in diameter, rose the highest; Mr. *Robins* intended to have made some more experiments, in order to a further discovery, what sized rockets would rise highest. But his engagements with the *East-India* company preventing him, Mr. *Samuel Da Costa*, late of *Devonshire-square*, a gentleman of an extraordinary genius in mechanics, and indefatigable in the application; Mr. *Banks*, a gentleman, who had for many years practised making rockets, and two other persons, undertook the prosecuting these enquiries; and having made several experiments, as well with regard to the composition as the length which rockets might be made to bear in proportion to their diameters, and of different sized rockets from one inch and a half to four inches diameter, they intended this winter to have made trials of some of yet greater diameter, had not the death of Mr. *Da Costa* prevented it.

I shall therefore beg leave to give some account of the success, which has hitherto attended this undertaking, so far as they went: and as it has been much beyond what was expected, I am in hopes, this short relation will not prove unacceptable.

Amongst some rockets fired in the last spring, there were two made by Mr. *Da Costa* of about three inches and a half diameter, which were observed to rise, the one to about 833, the other to 915 yards. At a second trial, made some time after, there was one made by Mr. *Da Costa*, of four inches diameter, which rose to 1190 yards. The last trial was made the latter end of *April* 1750, when 28 rockets were fired in all, made by different persons, and of different sizes, from one inch and a half diameter to four inches; the most remarkable of each size were as follows; one of

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one inch and a half rose to 743 yards; one of two inches to 659; one of two inches and a half to 880; another of the same size, which rose to 1071; one of three inches to 1254; one of three inches and a half to 1109; and one of four inches, which after having rose to near 700 yards, turned, and fell very near the ground, before it went out. These were all made by Mr. *Da Costa*. Besides these, there was one of the rockets of 24 inches in diameter, which rose to 784 yards, and another made by Mr. *Banks* of the same size to 833.

As the making of large rockets is not only very expensive, but likewise more uncertain than those of a lesser size; so from the last experiments it is evident, that rockets from two inches and a half to three inches and a half diameter, are sufficient to answer all the purposes they are intended for; and I doubt not may be made to rise to a height, and to afford a light capable of being seen to considerably greater distances than those before mentioned.

Before I conclude this account, it may not be improper to take notice, that, though the heights of the rockets are set down to a single yard, it is not pretended, the method made use of (though sufficient for all the purposes of these experiments) is capable of determining the heights to so great an exactness; for, as they were measured by only one observer, it is evident, that if any of the rockets deviated from the perpendicular, so as either to incline towards the place of observation, or to decline from it, the height would be given either greater or less than the truth; but as the base, upon which they were measured, was 1190 yards, the greatest error, that can arise on this account, will be but very inconsiderable. If we should suppose, there might be an error of 30, or even 50 yards, which is very highly improbable; it must then be allowed, that several of these rockets rose

to

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to 1000 yards, one to 1100, and another to 1200 yards, or double to any of those fired in the *Green-Park*.

I have been informed, that the relation of this affair has appeared so very extraordinary to some gentlemen conversant in such matters, that they have mentioned it as their opinion, that there must certainly have been some mistake, either in placing the instrument taking the heights, or otherwise. In answer to which I would observe, that, in all the experiments mentioned in this paper, the heights were all taken by the same person, viz. Mr. *John Canton*, and that the last trial was made in the presence of several very worthy members of this Society. That the instrument being first fixed to a proper angle, was not altered during the whole time of trial; and therefore, if there had been any mistake in fixing it, that mistake would have varied the height of all the rockets as much as those of Mr. *Da Costa's*; but it was those of Mr. *Da Costa* only, and that at three different trials, which rose to such extraordinary heights; and therefore, I think, we have sufficient reason to conclude, that their measures were certainly taken very near the truth.

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After the foregoing sheets were printed off, a copy of Mr. *Robins's* discourse about rifled barrels, mentioned above, was communicated to me by my friend Dr. *Broklesby*, physician to the army; he having received it from colonel *Draper*, whose martial achievements in the *East-Indies* have added a farther lustre to the character of the polite scholar and fine gentleman.

This discourse was read before the Royal Society immediately after that at page 218, and is as follows.

OF THE

NATURE AND ADVANTAGE

OF

RIFLED BARREL PIECES.

HAVING treated at large in the preceding papers of the numerous irregularities, which take place in most of the operations of gunnery by the deflection of the projectiles from their first direction, which, as we have seen, is occasioned by their whirling motion; it is now but reasonable to consider of the most effectual means for preventing these troublesome and perplexing deviations. But before I offer any methods of my own for this purpose, it is proper to describe a practice, which has long prevailed in several parts of *Europe;* and which, though in all probability originally intended for different ends, doth yet in many instances prevent the deflection here treated of; the producing of this effect being indeed the sole excellence, all its other boasted advantages appearing on examination to be only imaginary.

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The method I have here in view, and which I propose as the subject of the present essay, is that by rifled pieces: and these pieces, though well known on the continent, being but little used in *England;* it is necessary to give a short description of their make, and of the particularities, in which they differ from the common pieces. For which purpose I must observe, that the essential difference between them is this. That a common piece has its barrel smooth on the inside, whereas the rifled piece has its cylinder cut with a number of spiral channels; so that it is in reality a female screw, varying from the fabric of common screws only in this, that its threads or rifles are less deflected, and approach more to a right line; it being usual for the threads, with which the rifled barrel is indented, to take a little more than one turn in its whole length. The number of these threads in each barrel are different, according to the fancy of the workman, and the size of the barrel; and in like manner, the depth these channels, or rifles, are cut down to, is not regulated by any invariable rule; but differs according to the country, where the work is performed, or the caprice of the artificer. This is the general idea of a rifled barrel, as opposed to a common one; and the usual method of charging it (though there are different practices, which will hereafter be more minutely examined) is this. When the proper quantity of powder is put down, a leaden bullet is taken a small matter larger, than the bore of the piece was, before the rifles were cut; and this bullet being laid on the mouth of the piece, and being consequently too large to go down of itself, it is forced by a strong rammer, impelled by a mallet, and by repeated blows is driven home to the powder; and the softness of the lead giving way to the violence with which the bullet is impelled; that zone of the bullet, which is contiguous

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guous to the piece, varies its circular form; and takes the shape of the inside of the barrel: so that it becomes the part of a male screw, exactly fitting the indents of the rifle. And here it happens, that, when the piece is fired, that indented zone of the bullet follows the sweep of the rifles; and thereby, besides its progressive motion, acquires a circular motion round the axis of the piece, which circular motion will be continued to the bullet, after its separation from the piece; by which means a bullet discharged from a rifled barrel is constantly made to whirl round an axis, which is coincident with the line of its flight. And hence it follows, that the resistance on the foremost surface of the bullet is equally distributed round the pole of its circular motion; and acts with an equal effort on every side of the line of direction; so that this resistance can produce no deviation from that line. And (which is still of more importance) if by the casual irregularity of the foremost surface of the bullet, or by any other accident, the resistance should be stronger on one side of the pole of the circular motion than on the other; yet, as the place, where this greater resistance acts, must perpetually shift its position round the line, in which the bullet flies, the deflection, which this inequality would occasion, if it acted constantly with the same given tendency, is now continually rectified by the various and contrary tendencies of that disturbing force, during the course of one revolution.

This perpetual correction of a deflective effort on the foremost surface of the bullet, in consequence of the revolution of the bullet round the line of its direction, may perhaps be exemplified, by considering what happens to a castle-top, whilst it spins upon its point. For it will be easily acknowledged, that this, without its revolving motion, could not continue for the least portion

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of time in that situation. And if we examine, how this happens, we shall find; that, though its centre of gravity is not exactly over the point, it spins on; yet that inequality cannot instantly bring it to the ground according to its natural effort; because, during one revolution, the centre of gravity preponderates on every side of the top; and thereby raises it as much in one place, as it depressed it in another. And this reasoning (supposing that the tendency of the centre of gravity of the top to descend, be analogous to the action of the unequal resistance on the foremost surface of a bullet fired from a rifled barrel) will easily explain how, notwithstanding that inequality, the bullet keeps true to its track without deflection. And what is here advanced, is farther confirmed by the general practice with regard to arrows. For it is well known to every archer, that the feathers of an arrow are placed in a spiral form, so as to make the arrow spin round its axis, without which it would be obvious to the eye, that the arrow undulated in the air, and did not keep accurately to its direction. And it is owing to the same principle, that every school-boy finds himself under the necessity of making his shuttle-cock spin, before he can depend upon the truth of its flight.

This is the general theory of the motion of bullets discharged from rifled pieces; and it is found by experiment, that their actual motions correspond very well with these speculations. For the exactness, which those, who are dextrous in the use of these pieces, attain to, is indeed wonderful; and that at such distances, that if the bullets were fired from the common pieces, in which the customary aberration takes place, not one in twenty of them could ever be traced.

But what occurs most wonderful in this affair, is, that a method so singular and so successful, and which hath been so long and generally practised

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tised in many parts of *Germany* and *Swisserland*, should have its theory so little understood, as it appears to me to have been. For by all, I have been able to recollect, I am fully satisfied, that neither the inventor of this method, nor the practisers of it, nor any of the numerous authors, who have written about it, have been at all apprized of the true and genuine advantages hence arising; but have constantly represented the intention of it to be very different, from what I have here described; and have supposed it to be attended with conveniences, which, by a long series of experiments, I know to be altogether imaginary. For the truth of what I here advance, it might perhaps be sufficient to appeal to those gentlemen, who have at any time examined artificers, or those skilled in the practice of these pieces, about the use and intention of the rifles. For I doubt not, but they have found, as I have done, that one or all of the three following reasons have been constantly alledged. Either that the inflammation of the powder was greater by the resistance, which the bullet thus forced into the barrel gave thereto, and that hereby the bullet received a much greater impulsion, than it would have done from the same quantity of powder in a common piece: or that the bullet by the compounding of its circular and revolving motion did, as it were, bore the air, and thereby flew to a much greater distance, than it would otherwise have done; or that by the same boring motion it made its way much easier through all solid substances, and penetrated much deeper into them, than if discharged in the common manner.

These are the reasons, which I have always heard urged upon this occasion. And, as a proof, that this is the light, in which those authors have considered it, who have purposely treated of the subject; I shall quote the latest, I believe, who hath

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hath written about it; and who appearing himself to be a practitioner, and to be extremely inquisitive and curious in every branch of this business, may be supposed to give the most authentic account of what was generally believed in this matter. The person I mean, is *John George Leutman*, fellow of the Imperial Academy of Sciences at *Petersburg*: in the acts of which academy he has published two dissertations. The first of them about the manner of forming these rifles. *De sulsis cochleatis ad datam distantiam tubis sclopetarum recte inducendis.* The second containing certain curious remarks and experiments on the use of rifled pieces. *Annotationes et experimenta quædam rariora et curiosa ad rem sclopetariam pertinentia.**

In the first of these tracts he gives the following account of the intention of these pieces.

Primus, qui hanc finxit in tubis formam, proculdubio eum habuit finem, ut globus per gyrum, ope cochlearum, inductum, aerem terebrando facilius penetret atque secet, prohibeatque, ne linea directionis, globum impellens, a recta nimium deflectat via, et tandem corpus resistens, ad quod tendit globus, vehementius feriatur et trajectetur, quando globus gyrando illud perterebrat.† Where, though one part of it seems to point out that advantage, which I have above asserted to be the only one attending this practice; yet by a more careful attention to the words, and by comparing them with what he says in other places; it appears, that they relate to some fancied convenience in the impulse of the powder, and not to the rectitude of the track, in which the bullet flies.

Now that none of the three foregoing reasons hold true in the use of these pieces; I have satisfied myself by numbers of experiments made with rifled

* Tom. IV. Ann. 1729. p. 265.
† Tom. III. Ann. 1728. p. 156.

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rifled barrels of various sizes. For in these experiments I have found, that the velocity of the bullet, fired from a rifled barrel, was usually less than that of the bullet fired from a common piece with the same proportion of powder. Indeed, it is but reasonable to expect, that this should be the case. For if the rifles are very deep, and the bullet is large enough to fill them up, the friction bears a very considerable proportion to the effort of the powder; and that in this case the friction is of consequence enough to have its effects observed, I have discovered by the continued use of the same barrel. For the metal of the barrel being soft, and wearing away apace, its bore by half a year's use was sensibly enlarged; and consequently the depth of its rifles diminished, and then I found, that the same quantity of the same powder would give to the bullet a velocity near a tenth part greater than what it had done at first. And as the velocity of the bullet is not increased by the use of rifled barrels; so neither is the distance it flies to, or its penetration into solid substances. Indeed these two last suppositions appear at first sight too chimerical to merit a formal confutation. But I cannot help observing, that those who have been habituated to the practice of these pieces, are very excusable in having given way to these prepossessions. For they constantly found, that with them they could fire at a mark with tolerable success; though it were placed at three or four times the distance to which the ordinary pieces were supposed to reach. And therefore, as they were ignorant of the true cause of this variety, and did not know, that it arose only from preventing the deflection of the ball; it was not unnatural for them to imagine, that the superiority in the effect of the rifled piece was owing either to a more violent impulse at first, or to a more easy passage through the air.

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This may suffice as to the general idea of the form and convenience of a rifled piece; and before I enter into a detail of the varieties in its fabrick, and manner of charging it, or engage in any minute discussions relating thereto; it will be expedient to insert some experiments, by which it will appear, how well it answers the purpose, I have mentioned above; I mean that of keeping the ball to its regular track, by preventing that deflection, which, as we have seen, takes place in the bullets fired from common pieces.

And first I considered, that in consequence of the reasoning about the manner, in which it produces this effect; it should follow, that the same hemisphere of the bullet, which lies foremost in the piece, must continue foremost during the whole course of its flight.

To examine this particular, I took a rifled barrel carrying a bullet of six to the pound; but instead of its leaden bullet, I used a wooden one of the same size, made of a soft springy wood, which bent itself easily into the rifles without breaking. And firing the piece thus loaded against a wall at such a distance, as the bullet might not be shivered by the blow; I always found, that the same surface, which lay foremost in the piece, continued foremost without any sensible deflection, during the time of its flight. And this was easy to be observed, by examining the bullet; as both the marks of the rifles, and the part that impinged on the wall, were sufficiently apparent.

Now, as these wooden bullets were but the sixteenth part of the weight of those of lead; I conclude, that if there had been any unequal resistance or deflective power; its effects must have been extremely sensible upon this light body; and consequently in some of the trials I made, the surface, which came foremost from the piece, must have been turned round into another situation.

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But again, I took the same piece, and loading it now with a leaden ball, I set it nearly perpendicular, sloping it only three or four degrees from the perpendicular, in the direction of the wind; and firing it in this situation, the bullet generally continued about half a minute in the air, it rising by computation to near three quarters of a mile, perpendicular height.

In these trials I found, that the bullet commonly came to the ground to the leeward of the piece, and at such a distance from it, as nearly corresponded to its angle of inclination, and to the effort of the wind; it usually falling not nearer to the piece than a hundred, nor farther from it than a hundred and fifty yards. And this is a strong confirmation of the almost steady flight of this bullet for about a mile and a half. For were the same trial made with a common piece, I doubt not, but the deviation would often amount to half a mile, and perhaps considerably more; though this experiment would be a very difficult one to examine, on account of the little chance there would be of discovering, where the ball fell.

But it is now time to mention the varieties of these pieces, and the different methods made use of in different places for charging them.

The most usual is, doubtless, what I have already recited, that of forcing a leaden bullet down the piece by a strong rammer driven by a mallet. But in some parts of *Germany* and *Swisserland*, an improvement is added to this practice; especially in the larger pieces, which are used for shooting at great distances.

This is done by cutting a piece of very thin leather, or of thin fustian, in a circular shape, somewhat larger than the bore of the barrel. This circle being greased on one side, is laid upon the muzzle with its greasy part downwards, and the bullet, being placed upon it, is then forced down the

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the barrel with it; by which means the leather or fustian incloses the lower half of the bullet, and by its interposition between the bullet and the rifles, prevents the lead from being cut by them. But it must be remembered, that in those barrels, where this is practised, the rifles are generally shallow, and the bullet ought not to be too large.

As both these methods of charging at the mouth take up a good deal of time; the rifled barrels, which have been made in *England* (for I remember not to have seen it in any foreign piece) are contrived to be charged at the breech, where the piece is for this purpose made larger than in any other part. And the powder and bullet are put in through the side of the barrel by an opening, which, when the piece is loaded, is filled up with a screw. By this means, when the piece is fired, the bullet is forced through the rifles, and acquires the same spiral motion as in the former kind of pieces. And perhaps somewhat of this kind, though not in the manner now practised, would be of all others the most perfect method for the construction of these sorts of barrels.

After what hath been said of the advantages of these pieces, I must make a few animadversions upon their defects. And in the first place I must observe, that though the bullet impelled from them keeps for a time to its regular track with sufficient nicety; yet, if its flight be so far extended, that its track is much incurvated, it will then often undergo considerable deflections. This, according to my experiments, arises from the angle at last made by the axis, on which the bullet turns, and the direction in which it flies; for that axis continuing nearly parallel to itself, it must necessarily diverge from the line of the flight of the bullet; when that line is bent from its original direction; and when it once happens, that the bullet whirls on an axis, which no longer coincides with

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the line of its flight; then the unequal resistance described in the former papers will take place, and the deflecting power hence arising will perpetually increase, as the track of the bullet, by having its range extended, becomes more and more incurvated.

This matter I have experienced in a small rifled barrel piece carrying a leaden ball of near half an ounce weight. For this piece, charged with one drachm of powder, ranged about 550 yards, at an angle of twelve degrees, with sufficient regularity; but being afterwards elevated to twenty-four degrees, it then ranged very irregularly, generally deviating from the line of its direction to the left, and in one trial not less than one hundred yards.

This apparently arose from the cause above mentioned, as was confirmed by its constant deflection to the left; for by considering, how the revolving motion was combined with the progressive one, it appeared, that a deviation that way was to be expected.

The best remedy, I can think of for this defect, is the making use of bullets of an egg-like form instead of spherical ones. For if such a bullet hath its shorter axis made to fit the piece, and it be placed in the barrel with its smaller end downwards; then it will acquire by the rifles a rotation round its larger axis; and its centre of gravity lying nearer to its fore part than its hinder part, its longer axis will be constantly forced by the resistance of the air into the line of its flight. As we see, that by the same means arrows constantly lie in the line of their direction, however that line be incurvated.

But besides this irregularity already treated of, there is another circumstance in the use of these pieces, which renders the flight of the bullets uncertain, when fired at a considerable elevation. For I find by my experiments, that the velocity

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of a bullet fired with the same quantity of powder from a rifled barrel, varies much more from itself in different trials, than when fired from a common piece.

This, as I conceive, is owing to the great quantity of friction, and the impossibility of rendering it equal in each experiment. Indeed, if the rifles are not deeply cut, and if the bullet is nicely fitted to the piece, so as not to require a great force to drive it down, and if leather or fustian well greased is made use of between the bullet and the barrel in the manner described above; perhaps by a careful attention to all these particulars, great part of the inequality in the velocity of the bullet may be prevented, and the difficulty in question be in some measure obviated: but till this be done, it cannot be doubted, but the range of the same piece at an elevation will vary considerably in each trial; although the charge be each time the same. And this I have myself experienced in a number of diversified trials with a rifled barrel piece, loaded at the breech in the *English* manner. For here, the rifles being indented very deep, and the bullet being so large as to fill them up completely; I found, that, though it flew with a sufficient exactness to the distance of four or five hundred yards; yet, when it was raised to an angle of about twelve degrees (at which angle being fired with about one-fifth of its weight in powder, its medium range is nearly a thousand yards) in this case, I say, I found, that its range was variable, although the greatest care was taken to prevent any inequalities in the quantity of the powder, or the manner of charging. And as in this case, the angle was too small for the first mentioned irregularity to produce the observed effects; they can only be imputed to the different velocities, which the bullet each time received by the unequal action of the friction.

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From all that has been said then about the use of rifled barrel pieces; it is sufficiently obvious, that whatever tends to diminish the friction of these pieces, tends at the same time to render them more complete; and consequently it is a deduction from hence, that the less the rifles are indented, the better they are; provided they are just sufficient to keep the bullet from turning round in the piece. It likewise follows too, that the bullet ought to be no larger than to be just pressed by the rifles; for the easier the bullet moves in the piece, supposing it not to shift its position, the more violent and accurate will its flight be. And to render this last article the more complete; it is necessary, that the sweep of the rifles should be in each part exactly parallel to each other. For then, after the bullet is once put in motion, it will slide out of the barrel without any shake, and with a much smaller degree of friction, than if the threads of the rifles have not all of them the same degree of incurvation.

The foreigners are so exact in this article, that they try their pieces as to this particular by a singular artifice. For they first pour melted lead into them, and letting it cool, they procure a leaden cylinder of perhaps two or three diameters in length, exactly fitted to one part of the inside of the piece; then if this leaden cylinder, being gently pushed by the rammer, will pass from one end of the barrel to the other without any sensible strain or effort, they pronounce the piece perfect; but if it any where sticks or moves hard, they esteem it defective.

From the nature of these pieces it is plain, that they can only be made use of with leaden bullets; and consequently cannot be adapted to the adjusting of the motion of either shells, or cannon-bullets. However from the same principle, whence these pieces derive their perfection, other artifices may

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may be deduced for the regulating the flight of these more ponderous bodies. On some of these methods, which have occurred to me, I have already made several experiments; and there are others, which I have more lately considered, and which appear to me to be infallible. But there are many reasons, why I should not now engage in a circumstantial discussion of this kind. I shall therefore close this paper with predicting, that whatever state shall thoroughly comprehend the nature and advantages of rifled barrel pieces, and, having facilitated and compleated their construction, shall introduce into their armies their general use with a dexterity in the management of them; they will by this means acquire a superiority, which will almost equal any thing, that has been done at any time by the particular excellence of any one kind of arms; and will perhaps fall but little short of the wonderful effects, which histories relate to have been formerly produced by the first inventors of fire-arms.

THE END.

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